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SOME QUESTIONS OF FACT AND POLICY CONCERNING PHONEMES

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The major portion of this article is devoted to a criticism of Twaddell's monograph 'On Defining the Phoneme.'[1] The criticism is adverse throughout. Still, if the result of negating a negative is positive, it may be justifiable to presume that if this is a contribution at all, it is of a constructive character. Some negative assertions are made in said monograph concerning the results thus far achieved in the physical analysis of speech sounds. If we do not misconstrue those assertions, they are at variance with the literature on the subject. We wish to present the evidence.

It should be definitely understood at this point, however, that we do not offer this evidence in behalf of any theory. We have no wish to propose an additional phonemic theory, nor to attack or defend the validity of any proposed heretofore. It is claimed in the monograph that some writers regard phonemes as 'mental realities', others as 'physical realities', and in opposition to these a *tertium quid* is advocated. We do not intend to participate in this controversy. On such metaphysical issues the only opinion we shall express is that no advantage can be derived from their intrusion upon the technical domain of our subject.

Let us consider first the negative assertions alluded to. Twaddell seems to imply in some instances that all endeavors to analyze sound waves corresponding to speech sounds have been virtually futile. The only achievements of which explicit cognizance is taken in his monograph are those pertaining to duration and intensity. He concedes that 'The factors of duration and intensity (amplitude) are of course very precisely determinable.'[2] If this reference is relevant to the context in which it appears, it obviously implies some accomplishments which are not explicitly taken into account. It suffices to consider

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[1] W. Freeman Twaddell, On Defining the Phoneme, Publications of the Linguistic Society of America, Language Monographs, No. 16.

[2] Ibid. 24.

that if precise determination of duration and intensity are of any concern to the phonetician, it must be possible to determine the duration and intensity of such components of speech as vowels or consonants. It follows that some data corresponding to these components must be determinable in the sound waves. As a matter of fact, it is possible to determine even by bare inspection what portions of an oscillogram correspond to the consonants and vowels recorded. On the other hand, it is not justifiable to say that duration is 'of course very precisely determinable'. For it is evident to anyone who has carefully observed an oscillogram of speech, or has critically measured duration by this means, that in most instances, if not in all, there is no precise demarcation between the curves of two successive sounds. Indeed, if it were otherwise, the oscillograph would be at variance at least with some facts of articulation. Measurements of duration are only adequately accurate, and occasionally even arbitrary.[3]

There are in the monograph other implicit acknowledgments of achievements comparable to the foregoing, but apparently they are all contradicted by the following final statement: 'The only legitimate conclusion at present appears to be that the presence of "phoneme-features" as positive, additive entities in the sound-waves is not demonstrable, and there is no reason to believe that it will be.'[4] The expression 'phoneme-features' alludes to various statements by Bloomfield to the effect that phonemes are 'features of sound which the speakers have been trained to produce and recognize in the current of actual speech-sound'.[5] What is meant by 'positive, additive entities' is not clear to the present writer. Of course, it would be unreasonable to expect precise specification in this instance, since Twaddell refers to something which, according to him, has not been found. Whatever the particulars referred to by those terms, it may be granted that the analysis of sound waves would satisfy the requirements of scientific inquiry, if it were possible to assert that in every instance in which a given sound is recorded oscillographically, one or more statements

[3] Cf. C. E. Parmenter and S. N. Treviño, The Length of the Sounds of a Middle Westerner, American Speech 10.129: 'The exact limits of the sounds were sometimes difficult to determine, particularly in the case of the voiced continuants. But by comparing the troublesome cases with other occurrences of the same sound, it was possible to arrive at fairly reliable approximations. In a few cases, such as the combination [ər], it was impossible to find a basis for division and the time of the two sounds was arbitrarily divided equally between them.'

[4] Ibid. 24.

[5] Leonard Bloomfield, Language 80 (New York 1933).

can be made which are *all* true of *all* the oscillograms of that sound, and which are not *all* true of any other sound of the same language. If it is possible at present to make this assertion for a certain number of sounds, to that extent Twaddell's account of the achievements of applied acoustics is at variance with the facts.

Reports of various successful experiments yielding results of the kind indicated above have appeared in sundry publications. I must own, however, that most of them are known to me only by reference. To abstract convincing data even from the few which I have read, would require more space than is here available. An extensive bibliography will be found in the one important book to which we shall confine most of our remarks.

We refer to a work by Agostino Gemelli, a psychologist, and Giuseppina Pastori, a biologist.[6] The method of analysis employed by these investigators is mathematical throughout, although various observations are made by mere inspection. That such observations can be made on an oscillogram is readily disclosed even from a cursory examination of the 88 plates in their work. It can be safely asserted that an individual who has never seen oscillograms can learn in one or two hours to recognize the curves of some sounds, and will be able to differentiate most of the voiceless consonants from the other sounds in their various recurrences, whether in oscillograms of words or of complete sentences. But the expert can detect even individual peculiarities. For example, Gemelli and Pastori state that every time that two of their subjects pronounce the vowels *a, i, u*, they obtain curves which are typical of the pronunciation of these sounds by these two individuals. Hence, given a number of oscillograms of the speech of various subjects, they can pick out by inspection the ones pertaining to each of these two particular individuals, provided, however, that they contain one of these three vowels.[7] Even from these few remarks, it does not appear that the 'laboratory situation' is as discouraging as Twaddell presents it.

Before attending to a more significant part of Gemelli and Pastori's work, it should be understood that I claim no competence to judge the validity of their methods. I can only point out what indications of reliability are apparent. One notices that the book represents several years of laborious research. Many of us have faith in mathematics when properly applied, and any result obtained by employing two different mathematical procedures inspires a great deal of confidence. Gemelli

[6] L'analisi elettro-acustica del linguaggio (Milan 1934).
[7] Op. cit. 29.

and Pastori reach their conclusions concerning the characteristics of the Italian vowels through the Fourrier method of harmonic analysis as employed in the Hamburg laboratory,[8] and the Vercelli method of analyzing undulatory phenomena, a method which is said to have amply proved its effectiveness in other fields of research.[9] Gemelli and Pastori adapted this method to the analysis of speech sounds with the aid of Vercelli.[10] According to them, the use of the Vercelli method guards against the objections which have been made concerning the arbitrariness of the assumption that the components of speech sounds are all harmonic.[11] Such is the evidence which persuades me to accept as accurate the comment made by the Bibliography Department of American Speech, wherein I read: 'This is to date the most thorough example of physical analysis of [sounds of] a language.'[12]

Twaddell claims that no constants have been found in successive oscillograms of the same sound. Gemelli and Pastori claim without reservations or provisos that they have found the same characteristics for a given Italian 'phoneme' (fonema), for example, the vowel *a*, in such variable conditions as in the articulation of an isolated *a*, in an *a* sung in different pitches, in a whispered *a*, in a stressed and in an unstressed *a* as elements of isolated words, or of complete sentences, articulated by male and by female subjects. They find that the phoneme *a* has special characteristics in each of these cases, that it differs in part according to the phoneme that precedes as well as according to that which follows, but certain characteristics of the *a* are always present. The same claim is made respecting other vocalic phonemes. With regard to the 16 consonants studied by them, it is stated that the curves of each consonant have a characteristic form (fisionomia). Hence, save rare exceptions, each consonant can be recognized by one whose eye has been trained to see their characteristics.[13] As in the case of the vowels, the visual data are accompanied by mathematical

[8] Ibid. 91

[9] Ibid 68

[10] Ibid. 91 (footnote).

[11] Ibid. 66, 67.

[12] American Speech 10.150.   (The brackets are ours.)

[13] 'Tout oeil exercé *lit* dans les oscillogrammes (à part quelques rares exceptions) chaque consonne comme chaque voyelle, ce qui prouve que la courbe oscillographique a une physionomie caractéristique même pour chaque consonne.' A. Gemelli et G. Pastori, Quelques recherches sur la structure des consonnes 99 (Pisa 1934). (From the French translation of a portion of their work.)

computations. However, it appears that the consonants were not studied in as many diverse conditions as the vowels.

In the light of these reports, it seems justifiable to question whether or not the monograph under consideration misinforms the readers. The erroneous appraisal of the accomplishments in the field of applied acoustics, and certain fallacious inferences which we shall presently consider, lead to Twaddell's conclusion that 'there is no reason to believe' that the physical analysis of speech sounds will ever yield desirable results.

One of the fallacies alluded to is as follows. It is argued that our 'acoustic records' must contain all that we look for; that the frequency-response of our recording devices is adequate to the demands of this research. It is assumed that what we seek has not been found to any extent worth considering Therefore, the conclusion is either that what we seek does not exist, or that, although it may exist, it will not be possible to demonstrate its existence. It is not clear to which of these alternatives the monograph commits itself. At any rate, assuming merely for the sake of argument that the highest quality of acoustic recording attainable at present fulfils the requirements of our inquiry, it may at once occur to us that the records may contain what we seek, but we may not possess at present effective means of finding it. Twaddell seems to disregard completely the question of means of analyzing sound waves after they are recorded. But that is precisely one of the chief problems. The mathematical analysis of oscillograms is extremely laborious. Objections have been made to various physical modes of analyzing sound. Can satisfactory devices be invented? It is rather unwarranted in this century to predict that all the possibilities of inventing any sort of physical devices have been exhausted.[14]

There is another fallacious conclusion based on the result of eliminating frequencies, as reported by Stumpf.[15] It is argued by the author of the monograph that if speech is understood after filtering out certain bands of frequencies, the phoneme features have not been reached by the distortion. But it is obvious that if a faulty transmission of connected discourse or even of complete words is understood, all that can

[14] Perhaps it was not long before or after Twaddell prepared his manuscript that the announcement of O. H. Schuck's recent invention, the sound prism, appeared in the Proceedings of Radio Engineers 22.1295–1310, Nov. 1934. It is a 'new rapid acting heterodyne wave analyzer', which enables the experimenter to make observations on the components of sound waves in one tenth of a second, and photograph their spectra at any given moment during the observation.

[15] Quoted by Twaddell 23, 24.

be safely inferred is simply that it has not been mutilated sufficiently
to prevent us from understanding it with the aid of the context.   Even
in an isolated word the sounds which precede and those which follow
the affected sound may enable us to recognize the word.[16]   Specialists
in such investigations evade the effect of the context by employing non-
sensical combinations of consonants and vowels in what they call 'articu-
lation tests'.[17]   But, after all, has any one claimed that the phonemes
of a language consist of just that which is sufficient to enable the native
to understand it?   Consider the case of a foreigner who has not mas-
tered the pronunciation of certain English sounds.   We can understand
him although he may substitute [z] for [d], [v] for [w]; his [r] may be
uvular, and there may not be a single vowel sound in his discourse
which is ever uttered by the native speaker of English.   Does the fact
that we understand him prove anything respecting English phonemes?
Obviously, the conclusive proof of whether or not English is distorted
by a foreigner is not the possibility of understanding him, but rather
whether or not he betrays a foreign accent.   Likewise, the proof that
the distortion purposely introduced into a transmission of speech
affects the phonemes is the fact that our ear can detect the difference.
If by using an upper limit of 2461 cycles Stumpf notices 'certain dis-
tortions in the front vowels and voiceless fricatives', as quoted by
Twaddell,[18] it proves conclusively that certain frequencies above 2461
cycles are necessary to transmit these sounds as Stumpf is accustomed
to hearing them.   In fact, all the reports of experiments known to the
present writer agree with regard to the presence of high frequencies in
the front vowels and voiceless fricatives.   Take the vowel [i], for
example.   There is agreement not only as to the presence of high fre-
quencies in this vowel, but also with regard to what we may call its
differentiae.   If the notation [i] stands for the highest front vowel of a

[16] A familiar instance is that of spelling a word in a telephone conversation.
We have to say 'p as in Peter', 's as in Sam', due to the fact that the telephone
does not transmit the high frequency characteristic of these consonants.   Twad-
dell says (24) that we understand speech transmitted by the telephone, that its
low frequency-response does not affect the phonemes.   But notice the artificial
manner in which the telephone operators have to repeat the words 'five' and
'nine' to verify what they hear.   Notice the difficulty of distinguishing conso-
nants with high frequencies over the telephone when the context does not help
much, as in the words 'fifth' and 'sixth'.

[17] One of these tests is described in detail by John C. Steinberg, Effects of Dis-
tortion upon the Recognition of Speech Sounds, The Journal of the Acoustical
Society of America 1.121–137 (1929).

[18] Ibid. 24.

given language, and LP and HP stand respectively for the frequency
of the lowest and the highest characteristic partial (formant, reinforce-
ment within a certain frequency zone) of any vowel in that language,
then in the vowel [i] the ratio LP/HP is a smaller fraction than the
ratio LP/HP in any other vowel in the language. When, as in the
work of Gemelli and Pastori, the characteristic frequencies are given in
terms of harmonics or ratios to the frequency of the fundamental, then
a ratio F/HP in [i] is less than the ratio F/HP in any other vowel, if F
stands for the same fundamental frequency of [i] and the vowel with
which it is compared. These statements hold without exception for the
data given on three languages by five investigators,[19] viz., on English by
Crandall, Miller, and Paget; on Japanese by Chiba; on Italian by
Gemelli and Pastori. These authors used different devices and methods
of analysis as follows: Crandall, electric recording and analysis; Chiba,
electric recording, method of analysis not stated; Miller, mechanical
recording (Phonodeik) and Henrici analyzer; Gemelli and Pastori,
electric recording and mathematical analysis; Paget, aurally with the
aid of certain devices serving as tuning forks. If any one questions
the validity of these results because every investigator does not give
the identical frequency, say, 2579 cycles for the high formant of [i],
he should be able to give valid reasons for expecting: (1) that there are
no variables in the resonance of the oral and nasal cavities, in spite of
all other individual differences; (2) that there are no variables with
regard to muscular tension, position of the tongue, and other circum-
stances which may be involved in the articulation of a vowel by differ-
ent individuals or by the same individual on different occasions; (3) that
the recognition of any vowel depends in every case on the presence of one
or more tones with invariable, absolute pitch; (4) that the formants

[19] The statement concerning the differentiae of the [I] as given above is not
made by any of the authors cited. It was inferred by us from their data. I do
not cite Harvey Fletcher (Speech and Hearing, New York 1929) because his
figures (58) are approximate averages of the results obtained by Stumpf, Miller,
Paget, Crandall, and perhaps his own observations. With regard to Miller, it
should be noted that he does not give two formants for all the vowels. Our state-
ment, therefore, holds strictly only for [i] and the three other vowels for which
the phonodeik showed two formants, but it is not in disagreement with the data
on the vowels for which Miller gives only one formant. The reports referred to
are: Irving B. Crandall, The Sounds of Speech 26, Bell Telephone Laboratories,
Reprint B-162-1 (Nov. 1935); Dayton C. Miller, The Science of Musical Sounds
(New York 1926); Sir Richard Paget, Human Speech (New York 1930); T. Chiba,
Research into the Characteristics of the Five Japanese Vowels (Yokohama 1931);
Gemelli and Pastori, op. cit.

should have an invariable frequency regardless of the frequency of the fundamental pitch in which the vowel is uttered; and (5) that frequencies and only frequencies are the data in terms of which the differentiae of the vowels can be stated. If the conclusions of Gemelli and Pastori are corroborated by future experiments, such expectations are at variance with observed facts.

We find, therefore, no justification for Twaddell's assertion that 'even with the acoustically relatively simple vowels the so-called "formants" are only proximately determinable, and within very wide ranges.'[20] It is not clear why the ranges should be expected to be narrow, nor do we know what is the criterion for considering vowels to be acoustically (aurally) simpler than consonants. Furthermore, in the present stage of our knowledge, it is not justifiable to infer from an aural criterion of quality what is to be expected in the physical analysis, nor to deduce from the latter what will hold for the former with respect to quality.

To consider whether or not some phonemes have already been defined in terms of physical data would take us too much afield, particularly as I am not certain that I know what is meant by 'defining' a given phoneme. Furthermore, in view of the lack of consensus of opinion on what a phoneme is, it seems advisable to suggest that before we speak of defining the phoneme we should attempt to determine whether we are all referring to the same sort of things when we speak of phonemes. If some of us refer to one kind of things, and others to various other kinds, we are obviously talking at cross purposes when we speak of defining *the* phoneme. For each writer, 'to define *the* phoneme' may ultimately mean 'to define what *I* call phoneme'. Roughly put, then, the question may be 'Who calls what phonemes?'

The possibility of further progress in the analysis of sound waves should be of indifferent concern to Twaddell, but it appears to be otherwise. He claims that his definition obviates the difficulties inevitably encountered 'if the phoneme is defined in terms of observable phenomena'.[21] But in spite of this important claim, he seems to deem it desirable to discard his definition in the event that the physical analysis of speech sounds should produce desirable results in the future. To quote his own words: 'Then, if the next decades bring a satisfactory definition of the phoneme from the laboratory, we could discard the abstractional and substitute the physical definition, without disturbing

[20] Op. cit. 24.
[21] Op. cit. 31.

the products of earlier linguistic work'.[22] What reasons could be urged for discarding the 'abstractional' definition? Before attempting to answer this question, let us consider some of the merits attributed to this 'abstractional' definition.

In the last section of his monograph Twaddell suggests various applications of his phonemes, and the results which can be expected. No one would question, it seems to me, that such results are desirable. In every field of research, so far as I am aware, the validity of a theory is proved by its conformity with observable facts. Twaddell expects that his conception of the phoneme will fulfil this requirement in historical linguistics more rationally than any other phonemes, except perhaps those of the Cercle linguistique de Prague. Furthermore, his definition is said to remove obstacles which heretofore have led to arbitrary classifications. It is too much to expect, I think, that the results of human behavior should fit into neat compartments when accurately measured or analyzed. But human behavior is not an exception. Think of biological classifications. Think of the classifications of the elements of matter as metallic and non-metallic.[23] At any rate, if Twaddell's definition of the phoneme leads to a perfect classification, no more than perfection could be expected from a definition grounded on physical data. What, then, would be the reason for discarding Twaddell's definition if it is found that the analysis of sound waves reveals facts corresponding to what we wish to call 'phonemes'? The answer might be: (a) that Twaddell's phonemes do not really offer all the advantages that he predicts; or (b) that we prefer to deal with 'realities' rather than with something which is said to be an 'abstraction' or a 'fiction'. Since the first possibility is contrary to Twaddell's claims, we must conclude that only a metaphysical consideration could lead him to discard his phonemic classification.

It seems pertinent to raise at this point a question of values. Which is more important, to deal with 'realities' or to avoid making arbitrary decisions? Might it not be advisable in answering this question to consider what is the ratio of the number of arbitrary decisions to that of defensible decisions? Arbitrary decisions in classifying data are made in some, if not all, of the natural sciences, as indicated above.

[22] Op. cit. 33, 34.

[23] Three of the 92 known elements are on the borderline between metallic and non-metallic, while a common and important characteristic of the elements, namely, their capacity for chemical combination, is not exhibited by 6 peculiar gasses. Cf. Bernhard Bavink, the Natural Sciences 3, Eng. trans. of the 4th Germ. ed. (New York 1932).

If it is so important to adhere to 'reality' it would seem advisable to accept as 'reality' the fact that it does not fall definitely into geometric boundaries. Twaddell obviously prefers to deal with what he regards as 'reality', but he seems to think that we are in a serious plight with regard to phonology, and out of despair proposes a makeshift, a 'fiction'. Still this 'fiction' is such that a definition grounded on physical data might replace it 'without disturbing the products of earlier linguistic work'. The contemplation of this possibility suggests a rather technical question. What is the difference between a 'fiction' that can be expected to conform with future laboratory data and the more familiar things we have heretofore called 'assumptions' or 'hypotheses'? The answer to this question is to be sought in the tenets of a certain school of metaphysics, as we shall presently suggest.

So far as I can see, it is precisely on metaphysical grounds that Twaddell discriminates against other definitions of the phoneme. For example, he says that his procedure in determining the phonemes of a given language is 'very much like the "phonology" of the Cercle linguistique de Prague'.[24] As investigators having to deal with empirical facts, we should like to know what are the technical advantages of Twaddell's phonology over that of the Prague school. We would be far more interested in that than in the question of whether or not the formal statements of this school imply that there is a mind. But Twaddell is more interested in showing that Troubetzkoy and the members of his school believe in 'mental realities'. Thus, he supposes that 'If the valuable and suggestive work of many members of the Cercle linguistique de Prague has not been wholly convincing to many students of language, it is (aside from its newness) because of the subjective mentalistic definition of units and a somewhat truculent denial of the relevance of phonetic analysis'.[25] It seems to me that Twaddell also could be charged, not with denials of the relevance of phonetic analysis, but of the value of any phonetic analysis other than that of articulation. The evidence is found in that section of his monograph entitled 'Articulatory rather than acoustic characterization of the phoneme'. It appears, therefore, that so far as we can judge by Twaddell's statements, the chief advantage of his phonology over that of the Prague school is that his vocabulary conforms with the metaphysical convictions of those who do not believe that there is a mind.

[24] Op. cit. 58.
[25] Ibid. 58.

Is it necessary to be a materialist in order to be an investigator? Can not the work of the scientist proceed as heretofore, regardless of our personal convictions upon such metaphysical questions? We can judge, it seems to me, whether a definition does or does not enable us to identify that which is defined. If a writer tells us, for example, that a phoneme is an unconscious X such that its conscious manifestation performs a certain function of the social mind, there is no need of assailing his definition on metaphysical grounds. Whatever his metaphysical convictions or ours, he must, if he is a reasonable man, concede us the right to ask him how can we identify an 'unconscious X', and how can we determine whether in any given case it performs such a function. On the other hand, if a writer says that the phoneme [f] is an unconscious X manifested in the sound produced by a labiodental-voiceless-fricative articulation, we may choose to overlook the vagueness of 'an unconscious X' and proceed to identify the sound by the rest of the definition. So long as the 'unconscious X' does not interfere with technical procedures, we can ignore it; if for no other reason, because we know that it would be futile to argue the question. We could propose as a professional policy not to introduce in our technical domain any controversial questions whenever there is no need of so doing. Such a policy is in evidence in various fields of research, whether due to professional habits, or to a recognition of its advantages. To be sure, in one way or another a man's metaphysical convictions may be manifested in his theoretic outlook. That is inevitable. But even then, if a controversy arises, the arguments can be confined to the probable conformity of the theory with the facts of observation, or it can be controverted on purely rational grounds. If rationality is not accepted as one of the tests of validity, it need scarcely be pointed out that we make but indifferent use of our time in arguing.

Twaddell does not evade metaphysical controversies. On the contrary, he adds a new one to those with which we have had to contend heretofore. If his proposal to regard the phoneme as a 'fiction' is not merely a matter of words, then, whether or not he is aware of it, he advocates the metaphysical views of the Philosophie des Als Ob. Most of us are not likely to recommend that in our quest for facts, truth, knowledge, or whatever it is that we are after, a problem be solved by means of a 'fiction'. To most of us fictions are things like mermaids and unicorns. To Hans Vaihinger, the founder of the philosophy of 'As if', the difference between mermaids, angels, spirits, etc. on the one hand,

and atoms and mathematical procedures on the other, is ultimately one of subject-matter.[26] In the very vocabulary of the philosophy of 'As if' Twaddell recommends a 'heuristic fiction' as a solution of our difficulties. Our reaction to his proposal, in so far as what is proposed is said to be a fiction, can well be expressed in the words of Vaihinger himself: 'We must be careful not to bar the road to verification by the use of the term *fiction*, and still less must we commit the more obvious mistake of labeling an assumption a *fiction* through sheer laziness, in order thereby to avoid the laborious task of verification.'[27] The dogma, doctrine, or policy of many scientists with regard to such metaphysical questions is that of a bio-chemist, author of a well-known book on scientific method:

> Any assumptions that are made for purposes of scientific investigation must be assumed as correct until they lead to contradictory results, then they must be corrected, but it is all a technical matter that does not concern metaphysicians acting in their official capacity. If some or all of the results of science are due to the constitution of the human mind we must just put up with the consequences until some non-human mind can explain to us how things really are.[28]

Twaddell's concern with the metaphysics of existence leads to what, in our opinion, are futile considerations. We are informed respecting the phoneme that 'it "exists" in the somewhat peculiar sense of existence that a brother *qua* brother, "exists"—as a term of a relation.'[29] I fail to understand why the existence of a 'brother, *qua* brother' is peculiar in any sense. It is difficult to see that the linguist can derive any benefit from taking into account the hypostases which puzzle some epistemologists. In our work, we can dispose of such metaphysical questions as the existence of a 'brother *qua* brother' by referring to the obvious fact that language abounds in, let us say, shorthand expressions. Thus, in one context the expression 'A and B are related as brothers' is

[26] 'However interesting these and other fictions, such as angels, devils, pixies, spirits, etc. may be for the logical theory of existential propositions, they are of minor importance for our present theme. At most they concern us in so far as such a judgment as "matter consists of atoms" or "the curved line consists of infinitesimals" is to be understood only as a fictive judgment in which no existence is predicated.' Then, referring to mathematical conceptions: 'All these concepts are contradictory fictions, mathematics being based upon an entirely imaginary foundation, indeed upon contradictions.' H. Vaihinger, The Philosophy of 'As if', Eng. trans. (New York 1925) 82 and 51, respectively.

[27] Op. cit. 87.

[28] A. D. Ritchie, Scientific Method 98 (London 1923).

[29] Ibid. 49.

equivalent to 'A and B were born of the same parents'. Here, the relation 'brother' seems to be decomposed into the relations 'and', 'same', 'parents', and various others, but the whole question seems to take the aspect of a historical fact. It either happened or did not happen, and the 'peculiar' mode of existence of a 'brother *qua* brother' seems to vanish like a phantom. In another context, 'A is B's brother' is equivalent to 'A should treat, love, protect, act toward, etc. B as persons born of the same parents are expected to do in their community'. Again, we cannot perceive any 'peculiar' existence. How brothers are expected to behave in a given community can be determined by a purely inductive research. If A and B were born of the same parents, we can express various facts by saying that there is a symmetrical relation of 'brother' between A and B. Likewise, if two words are historically derived from a third word, we can express various facts by saying that there is a symmetrical relation of 'cognate' between the two. Now, we may ask, is there a symmetrical relation *between* two books that were written by the same author? If it should appear to some one that there is no relation *between* the two books, or that one book has no relation *to* the other, or that the two are not related, it may be pertinent to consider whether we 'feel' that certain relations 'exist' only when a speech-community has developed a certain sort of linguistic device equivalent to one or more prolix expressions. No English word, so far as I am aware, is a shorthand expression for 'to have been written by the same author as'. No word expressing this fact can be substituted, in conformity with established usage, for R in an expression of the form 'This book is R to that book.' In short, we wonder how much of this whole question of relations *qua* relations is merely a matter of condensing one or more statements into what Twaddell calls a 'term of relation'. At all events, we fail to disclose any 'peculiar' mode of existence exemplified in any 'brother *qua* brother', nor in any phoneme *qua* phoneme. It either is something definitely known, or more or less vaguely known, or the whole thing is but a mere matter of words.

I must admit, however, that this opinion may be due to my inability to comprehend the 'nature' of existence or the 'nature' of nature. It would seem that in phonology a man cannot qualify as an investigator unless he is well versed in metaphysics. Some would say that such is the nature of the subject. According to my opinion, it may very well be the nature of our professional tradition. I do not know of any branch of knowledge in which so much concern is shown as in ours with whether the subject-matter dealt with is 'abstract', or is 'concrete' or

'real'.   In our professional vernacular the last two terms are apparently synonymous, and vaguely the opposite of 'abstract'.

Is there any universally accepted criterion by which we can determine whether something is abstract or concrete?   I know of only one which might be accepted, if it were of any use to do so.   It could be said that the 92 known elements of matter, and anything which is susceptible of chemical analysis is concrete, while everything else is abstract, with provisional exceptions in the domain of physics.   We would let the physicist decide whether atoms, electrons, photons, etc. fall under one or the other category.   On this basis a sound wave may be as real and definite as anything can well be, but it is not concrete.   The physicist says that it is a succession of condensations and rarefactions of the air. It is not the particles themselves, but their motions toward and away from one another.   Accepting for the sake of argument that motion is, as Twaddell remarks,[30] change of spatial relations, we may ask whether change, space, and relations are not abstract.   Furthermore, if what is 'abstract' is fictitious as Twaddell might perhaps assert with Vaihinger, hardly anything is more fictitious than mathematics.   Yet, the success of the physical sciences is attributed, at least in part, to the use of this 'fictitious' tool.   How much of what the linguist deals with can be defined as concrete?   I suspect the only concrete things we deal with are the so-called organs of speech, and various indicative and representative marks, photographs, phonetic symbols, kymograph tracings, and the like.   To be sure, in a different sense of the term, we deal with many things which are concrete, and we even resent being told that we have no 'concrete' ideas upon some subjects.   I suspect that the question of whether phonemes are 'abstract' or 'concrete' may be found to be nonsensical if properly analyzed.

In conclusion, I must admit that by delving in the subjects whose extrusion is here advocated, I have been inconsistent with my own policy.   If this inconsistence is excusable at all, it may be in virtue of the fact that sometimes the end justifies the means.

[30] Ibid. 37 (footnote).

PHONETIC AND PHONEMIC CHANGE

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The theory of phonemes has received a great deal of attention from linguists within the last few years, and the applications of the theory to descriptive linguistics have become reasonably clear, though there still remain problems in need of further study. However, there has been less interest in the theory on the part of students of the history of language, many of whom practically ignore the theory altogether; or assume, on the other hand, that the sound changes which we can prove to have taken place in the past were practically coextensive with phonemic shifts.[1] It seems, therefore, worth while to attempt a tentative examination and classification of the relations between sound change and phonemic change in the hope that the applications of the theory of phonemes to the historic study of language may be to some extent clarified.

In the first place it is clear that the shifts in the pronunciation of a whole set of phonemes, which leave the phonemes as far apart as at the start of the change, involve no shift in the phonemic pattern. This is usually clearly recognized by all scholars, and involves no particular difficulty. A simple example is the fact that most phoneticians who visit the Southern United States for the first time, or after a stay in New England, are struck by the fact that the whole Southern vowel scheme is a notch higher than that of other parts of the country. Aside from individual differences in the pattern of vowel phonemes due to other causes there are, however, no striking differences in phonemic pattern in the two dialects. Historically, also, the first consonant shift did not result in extensive phonemic changes, since the three groups of IE sounds remained separate in PGmc., though on a new basis.

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[1] This seems to be the attitude of Bloomfield, Language, N. Y., 1933. In his chapters on Phonetic Change and on the Comparative Method he makes quite clear that in general the formulae in which historic states of language are summed up indicate historic phonemic rather than narrowly phonetic structure. He does not, however, give any detailed discussion of the exceptions to this rule, or of the relations of the two types of change.

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Allied to this principle is a second, less often noticed. Phonetic shifts can result in a change in what constitutes the significant element in a phoneme or set of phonemes. This likewise produces no change in the phonemic pattern, though, as in the type of change cited above, the individual phonemes affected are all altered. An example of this type of change is found in the history of English long and short vowels. Originally the long and short vowels of English seem to have had the same quality; thus the significant feature of the two sets must have been length; and that the difference was phonemic is proved by such OE pairs as *fullīce* 'fully, completely', and *fūllīce* 'foully, basely'. However, in the late 13th century in the North, and a little later in the Midlands,[2] a change set in the quality of the short vowels, whereby the natural tendency toward relaxation in short sounds resulted in open quality, eventually giving rise to the distinction that exists between such modern phonemes as [u] and [ʊ]. The gradual result of this sound change has been that quality has replaced quantity as a mark of distinction between phonemes in Modern English. The phonemes remain at equal distance from each other, but the nature of the difference has changed. It is interesting that this change has indirectly contributed to a new treatment of length. Since length has become a non-distinctive feature the way has been left open for a regrouping of long and short quantities in rigid correspondence with the phonetic situation.[3] In fact I think most of the puzzling changes in the quantity of Modern English vowels can be referred to this general tendency, rather than to elaborate 'laws' describing the treatment of individual long and short vowels in special situations, as was the method of many older grammarians.

A third type of phonetic shift which need not involve phonemic readjustment is combinative sound change. Combinative sound change does not necessarily produce a phonemic shift so long as the sound causing the change remains. The simplest example of this sort of assimilation, not resulting in phonemic difference, is to be found in the almost universal American treatment of the vowels of *pat* [pæt] and *pan* [pæ̃n]. In spite of the nasalization of *pan*, the two vowels are still members of the same phoneme, and it usually requires training in phonetics for an American speaker to perceive the phonic difference. If, however,

[2] Cf. Luick, Historische Grammatik der Englischen Sprache 374-9, Tauchnitz, Leipzig, 1921.

[3] The best descripton of the phonetic rules governing NE vowel length is to be found in E. A. Meyer, Englische Lautdauer, Uppsala, 1903.

the two final consonants should disappear, while the nasality persisted, the two vowels [æ] and [æ̃], would then automatically become members of different phonemes.

An almost exactly similar assimilation which does not affect the phonemic pattern occurs in the speech of Spaniards, who often nasalize the first vowel of a word like *notario* without recognizing any difference between it and the last vowel of the word. A further example is the velarization of the nasal consonant of Spanish *cinco* [θiŋko], which is still felt to be a member of the *n* phoneme. Another case is the French unvoicing of the final *m* of words like *rhumatisme* [Ɦymatism̩].

A more complicated example of the principle that as long as the sound which causes the assimilation remains, the phonemic pattern is not affected, is found in Modern English. Thus in my own speech the Early Modern English open [o] before [r] has resulted in a long [ɔ]-like phone. Since I use this phone in all words of the [o + r] type, and since the [r] is always preserved, I make no distinction between pairs like *coarse* and *course*, *morning* and *mourning*. This open phone is therefore still a member of the [o] phoneme, though the change which produced the variant phone is of considerable antiquity. In the speech of Virginians and New Englanders who do not pronounce final and preconsonantal *r*, on the other hand, the loss of the sound which brought about the lowered variant of the phoneme has resulted in the setting up of a new phoneme, since *foe* and *for* now contain significantly different sounds, not phonetically controlled variants. In a phonemic transcription of such a type of speech *foe* and *for* would have to be distinguished by some such symbols as [fo] and [fɔə].

The difficulty of determining whether a historic sound change represented a mere phonic shift, or is one involving phonemic difference, is sometimes considerable. Thus the OE change of 'breaking' is a case in point. OE *æ* was 'broken' to a diphthong, written *ea*, and presumably pronounced [æə], before double *ll* as in [*fællan] > [fæəllan]. The generally accepted cause of this breaking was the development of a glide vowel before a 'dark' variety of *l* in the same syllable. If the [æə] had continued to occur only before such dark sounds, it would clearly have remained only a subsidiary member of the [æ] phoneme, in spite of the different spelling. There were, however, other varieties of *ll*, particularly a brighter *ll* from WGmc. *lj*, before which breaking often did not take place.[4] The occurrence, however, of occasional forms like Northern *sealla* from WGmc. **saljan* suggests that perhaps

[4] Sievers-Cook, Grammar of Old English 51, Ginn and Company, N. Y., 1903.

the diphthong was carried over to the bright varieties of *ll* as well, in which case phonemic readjustment of either the vowel or the consonant must have taken place.

Occasionally we are more fortunate in being able to determine the phonemic standing of an ancient sound change. Thus the Gothic lowering of Gmc. *u*, represented in Gothic by the spelling *au*, occurs only before *r*, *h*, and *hw*. There is, therefore, no evidence that it was not still a member of the *u* phoneme, since it is obviously phonetically controlled.

Even without the loss of the sound which brings about change in the members of a phoneme placed in its neighborhood, phonemic shifts may, however, occur. Such phonemic shifts nevertheless do not result in the setting up of new phonemes. Their sole result is to bring about a redistribution of already existent phonemes in the words affected. What happens in such a case is that the combinative sound change produces a phone which is closer to a member of some other phoneme than it is to the original phoneme, with the result that attraction sets in, assimilating the aberrant phone to this new close neighbor. This is what has happened in forms like [kæpm̩] for older [kæptən]. On a larger scale it is illustrated by those people who, while still preserving final and preconsonantal *r*, refer the [o + r] words to the same phoneme as *law*, making no distinction between *war* and *wore*. An instructive example of phonemic regrouping of this sort is cited by Grammont.[5] He points out that in the French phrase *robe courte* the final *b* of the first word is unvoiced, but remains a member of the *b* phoneme, since it is still a lenis. In *obtenu*, on the other hand, the following voiceless sound is a constant part of the environment, not a mere accident of the phrase. Here, therefore, the unvoiced *b* goes completely over to the *p* phoneme, since it loses its lenis quality.

To turn to phonetic shifts which involve shifts in the phonemic pattern. The most obvious of these is a shift in the direction of some already existent phoneme. Such a change results in the falling together of two phonemes, eliminating one from the total number. A well known example of such a falling together is that of ME [e:] as in [kwe:nə] and [ɛ:] as in [hɛ:θ] which have both given rise to NE [i:] as in *queen* and *heath*. A type resulting in an increase of the number of phonemes by means of a split in what was once one phoneme has already been discussed; that is, the splitting of a phoneme because of the loss of a sound which caused a combinative change, as in [fo], [fɔə], above.

[5] Grammont, Traité de Phonétique 186–7, Delagrave, Paris, 1933.

Here it is only necessary to add that it is sometimes difficult to determine when the change-causing sound can be called lost. This is particularly true when complete assimilation takes place. Thus it was mentioned that WGmc. *lj* resulted in a double *ll*, as in WGmc. **taljan* > OE *tellan*. Can one speak of this *j* as being lost if the second *l* is still its representative? The answer should be in the affirmative, since we have essentially one sound, though long, as the representative of the two earlier ones. Moreover it is probable that this *ll* was different in character (perhaps palatalized?) from the common Germanic *ll*, since the geminated *ll* did not produce breaking in OE. As long, therefore, as the difference in character persisted, the two *ll*'s constituted separate phonemes.

No less important than these purely phonetic changes are changes in the phonemic pattern resulting from dialect mixture. Thus if a given local dialect shifts a whole phoneme in a new direction (without thereby bringing about a collision with some other phoneme) no phonemic shift occurs. However, if the speakers of that dialect thereafter come in contact with another dialect, or a standard language which they imitate, phonemic changes almost certainly result. If phonemic changes occur, the result may be either the setting up of a new phoneme, or a redistribution of already existent phonemes. The only instance in which such dialect mixture does not produce phonemic changes is when a true variphone is set up, each speaker using either of the dialectically variant phones in all of the words in question. Such cases would seem to be rare. A more common state of affairs is fixation of the variants, either in different ones of the affected words, or in different senses of the same word. An interesting example of the setting up of a new phoneme by fixation of the second type is found in the speech of a subject from eastern Maine. This speaker comes from a region in which the [o] phoneme is strongly centralized, giving a phone which can be written [ɵ]. However, this particular subject belongs to a family in which local dialect has long been abandoned for Standard English. Thus *coat* has the [ou] phone of the Standard English phoneme. But in the special, and more homely, sense of *coat of paint* the local phone [ɵ] occurs. Thus there has been a phonemic split, actually in two senses of the same word.

The type of shift which results in the redistribution of already existent phonemes can be exemplified from the speech of many Virginians. The local dialect of eastern Virginia has the [a] phoneme in words of the *aunt, dance* type. However, the [æ] phoneme characteristic of General American occurs in the special use of *aunt* or *auntie* as a title for an old colored woman. It now seems probable that the puzzling divergences

in the history of ME [ʌ] and [ʊ], resulting respectively in NE [a] as in
father and [æ] as in *rather*, [ʊ] as in *put* and [u] as in *but*, but with fixations
in individual words, are the result of dialect mixture of the types de-
scribed above; the first resulting in redistribution of existent phonemes,
the second giving rise to a new phoneme.

The case of genuine variphones is unknown to me in personal experi-
ence of dialect investigation.   However, I am acquainted with speakers
who vary separate phonemes in a way almost exactly similar.   Thus
one subject who has lived both in Virginia and the middle Atlantic
states uses either [æ] or [a] phonemes in words of the *aunt, dance, ask,*
type, according to what sort of speakers he is addressing.   Also the
border-line cases in which some words occur with either phone, but some
are limited to one or the other, is known to me from subjects in Maine.
Thus one speaker can say either [stoun] or [stɵn], but always says [bout].
In such types of distribution it seems closest to the truth to say that
there are two phonemes, but that some words can be pronounced with
either one.

There remain fairly numerous examples of change in pronunciation
where there is a phonemic shift without any general sound change.
This is the type of change found in individual words, in which the
change is not supported by a similar drift in other words of the same
history.   An example of this kind of individual change is found in the
speech of some localities in Virginia where the single word *say* has under-
gone lowering in the stressed position so that it appears with the pho-
neme of *bed*.   This is in contradiction to the general tendency in this
region, which is to raise rather than lower vowels in stressed syllables.
Another similar example is found in Maine where at least one speaker
says that an ox is driven with a [gɔəd], using the phoneme of *board*.   It
is highly improbable that such an individual change, of whatever origin,
should result in the setting up of a new phoneme, occurring only once
in the speaker's language, since it can usually be assumed that the
attraction of existent phonemes will be too strong for a single word to
resist it.   Only in the more or less isolated and sub-linguistic forms of
interjections and similar highly colored words can phones not found
in the general phonemic pattern easily maintain themselves.   The
origin of such individual changes as the two cited above is extremely
various, and must always be explained out of a knowledge of the history
of the individual word in question.   Thus, though I cannot prove it,
I suspect that the first change is due to the analogy of the third singular
says, and the preterite *said*.   As to the second example, the explanation

would more probably be found in the existence of the words *gore, gored, gourd,* which have somehow become blended with *goad,* always a word of limited application in folk speech. Thus analogy, folk-etymology, and the restoration of worn-down forms all play an important part in such individual changes.

Closely allied to the phenomena of individual phonemic attraction are certain other phenomena that result from closely similar or overlapping phones within two separate phonemes. In English, a characteristic change is the reduction of many unstressed vowels to [ə]. When any of these reduced vowels are restressed, an 'incorrect' form, i.e. not belonging to the original phoneme, may be the result. Such a form is the Louisiana ['pʌkɔn] for *pecan.* More interesting, and less widely known, however, are cases of individual attraction resulting from contiguity of two whole phonemes. Thus in many parts of New England there are raised and fronted variants of the [a] phoneme, producing a phone, [aᴧ], which is extremely close to the lower limits of the [æ] phoneme. In general the two phonemes are kept quite distinct, but in occasional words attraction between the two phonemes has produced confusion. A clear instance of this kind of confusion is found in the speech of a subject from southern Massachusetts, who pronounces the first syllable of *clapboard* with the [aᴧ] phone, which is a member of the [a] phoneme, not the [æ] phoneme.[6]

From the preceding discussion it seems possible to deduce a few general principles which govern phonemic change in its relation to phonetic change. The most important of these is that when phonetic change brings two phonemes close together, attraction may set in. This attraction may manifest itself more strongly in some words than others, thus appearing at first as a confusion between phonemes of individual words, though the two similar phonemes may elsewhere remain distinct. Later the attraction may extend to the whole group, in which case we say that 'two sounds have fallen together'. Indeed, I think it may safely be assumed that no two phonemes ever fall together without passing through such a transition stage, in which the attraction manifests itself sporadically in ever increasing numbers of words.

However, attraction is not the only possibility when phonetic change brings two phonemes close to each other. A second possibility is phonemic repulsion, which results in the selection by the speaker of

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[6] The pronunciations [kætrɪdʒ] for *cartridge,* and [pæsl̩] for *parcel,* common in New England, are not examples of the reverse confusion, but of early loss of [r] before dentals.

variants which offer less overlapping, and so tend to increase the distance between the phonemes, rather than to lessen or eradicate it. An instance of phonemic repulsion seems to have taken place in the history of British English in early modern times. We know that at one time some varieties, at least, of London English had a voiced intervocalic *t*, which we can assume was probably the same phone as the flapped, voiced *t* so common in America at present.[7] We also know that at some time, presumably fairly recent, British English must have developed the flapped intervocalic voiced *r* which is characteristic of Standard British English today. These two phones are extremely close to each other, differing principally only in length. Consequently if they existed as members of separate phonemes at the same time, we should expect confusion to have arisen. Evidence of this confusion, and so of the contemporaneity of the two phones is found in *porridge*, the by-form from *pottage*. But at present British English *t* is unvoiced, and even slightly aspirated, medially, so that there is no longer the slightest danger of confusion with *r*. I know of no explanation other than habitual selection of variants farther removed from *r* than is the flapped *t* to account for this drift.

As to whether attraction or repulsion will result from the overlapping or contiguity of two phonemes, it seems to me that we must resort to the principles of dangerous as against unimportant confusion laid down by the French linguistic geographers to account for the disappearance or preservation of homonymous words. Thus it is demonstrably more important to keep consonant phonemes clear and distinct in English as it is at present organized, than it is to do the same for vowel phonemes. It is thus not strange that confusion in consonants should be rarer than in vowels, where several ME sounds have been confused in NE.

In conclusion, the theory of phonemic attraction and repulsion, if accepted, should modify considerably our notions of how linguistic change takes place. Where the neo-grammarians held that the individual speaker was without control over sound drift, the theory of phonemes emphasizes that many more things than inexorable phonetic law can control the non-distinctive features of utterance. The selection of those phones within a given phoneme which offer least likelihood of confusion may often spring from a necessity for clearness which has too often been ruled out of court by students of language.

[7] Cf. the spellings collected by Wyld, A History of Modern Colloquial English 312–13, E. P. Dutton, N. Y., 1920.

VENETIC *tolar*, OLD IRISH *canar*, AND THE INDO-EUROPEAN INJUNCTIVE

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[In Venetic inscription no. 249, Ethnographic Museum of Vienna, the *r* in the second line goes with *tola* in the first and *tolar* is a verb, third singular present middle, and not a noun. It belongs with OIr. *canar* and has an indicative and personal meaning 'he dedicates'. *canar* means in the Old Irish glosses 'is sung', not 'there shall be singing'. The comparison of Old Irish, Latin, Oscan (**fufans**), Venetic, Greek (ἐτλᾶν), Lithuanian, Old Ch. Slav., and perhaps Tocharian, indicates that this *ā*-formation in PIE was essentially indicative. Its 'injunctive' value is simply a modal use of the indicative.]

All linguistic scholars must welcome most warmly the appearance of The Prae-Italic Dialects of Italy by Conway, Johnson, and Whatmough, which renders accessible a mine of authentic information for the study and interpretation of these interesting ancient dialects. In the introduction to Part I (The Venetic Inscriptions), Conway remarks[1] that 'The remains of the Venetic language, though scanty, are of considerable importance to Indo-European philology, since it is clear that they represent a type of inflexion in many ways intermediate to that of Greek and Latin; for example, the verbs have no augment in past tenses, but they preserve the ending *-to* in the 3rd pers. of the middle voice. Venetic presents also some characteristics which resemble those of the Italic Dialects in the strict sense of the term, i.e., Oscan and Umbrian.'[2]

Of especial interest among the Venetic inscriptions is no. 169; because, if my interpretation of it is correct, it contains a verb. Verbs in Venetic, so far as it has been preserved to us, are very scarce. With the exception of the formulaic *zonasto* 'donavit', which occurs *passim* in dedications, and its equivalent *zoto*, which is found some six times, only three verb-forms, not including the one which forms the subject of this paper, are listed by Conway in his Outline of Venetic Grammar.[3]

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[1] The Prae-Italic Dialects of Italy, R. S. Conway, S. E. Johnson, J. Whatmough, 1. 5 (Cambridge, Mass., 1933).

[2] On the position of Venetic see P. Kretschmer, Einleitung in die Geschichte der griechischen Sprache 271 (Göttingen 1896); F. Sommer, IF 42.132.

[3] Prae-Italic Dialects 1.190.

Extreme interest therefore attaches to a form which may reasonably, and as I think quite certainly, be interpreted as a verb. It has what is known as 'scarcity-value'.

The text of our inscription (no. 249 in the Ethnographic Museum at Vienna; Pauli[5] 291) is written *boustrophedon* along two edges of a bronze tablet, for a description of which and its provenance reference to Conway[4] will suffice. It reads:

<div align="center">

ve·n·natola
·r·maχetḷo·n·

</div>

On this text Conway, 175, comments as follows:

'If the reading is sound, the beginning is naturally connected with **ve·n·noni·s·** in Padua, 144 *sup.*, and the *gens Vennonia* frequent in the Venetic area;[6] but if so, where does the second word begin, and end? The last word is probably a neuter noun describing the object (*armarium*?) to which the plate was affixed.' And Whatmough adds a note: '-*ar* forms (nom. sing. neut.?) are common in Messapic (e.g. **domular**) and Sicel (e.g. σῦφαρ).

As regards the answer to Conway's query, there seems to me no reason to doubt that the first word is *Venna*, a proper name. So Pauli (Veneter 411, 254) and Sommer (IF 42.112). This gives us the beginning of the second word. That the last word is *maχetlon* seems also probable, though its meaning is obscure.[7] I agree with Sommer in considering a neuter in -*on*, as Thurneysen suggested, containing the IE suffix of the instrument -*tlo*-, more likely than a peculiarly-formed proper name.[8] The end of the second word is then *r*, a well-known IE verbal termination.

Pauli, to be sure, seems to consider the possibility that we might take the *r* by itself, leaving *tola* as the second word (l. c. 254 'wenn das *r* für sich zu nehmen'). But that is clearly impossible, since an *r* by itself would be meaningless. Pauli was doubtless misled by his lack of understanding of the significance of the Venetic double puncts, which Conway considers to be signs of the accent.[9] The fact that the last letter of

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[4] Ibid. 1.170.

[5] Carl Pauli, Die Veneter und ihre Schriftdenkmäler, volume 3 of Altitalische Forschungen (Leipzig 1891).

[6] Conway's reference (134) for the *gens Vennonia* should be Part II vii C. Cf. Pauli, Veneter 310; Thurneysen, WklPh. 1892.289.

[7] Cf. Sommer, l. c.: 'Was das Wort bedeutet, wissen wir nicht; ebensowenig, was in dem voraufgehenden *tola·r·* steckt.'

[8] Pauli, 326 f.; cf. Sommer's criticism, IF 42.116–7.

[9] Prae-Italic Dialects 1.191 ff.

tola·r· is run over and stands by itself in the second line should give us no concern, since the Veneti,[10] like the Etruscans, were in the habit of dividing words between lines after any letter they pleased, without regard to division of syllables.[11]

Whatmough's suggestion that we might have in *tolar* a nom. sing. neuter noun does not seem to give any plausible interpretation of the inscription. We already have one neuter noun in the text, *maχetlon*, and I do not see what we should do with another. *maχetlon* we should naturally take as accusative and consider an appellative indicating the name of the offering[12] and *Venna* is doubtless in the nominative[13] and represents the donor. What we need is not another noun, but a verb.

Pauli, in fact, takes the second word of the inscription as a verb, but not, I suppose, being able to give a satisfactory interpretation of *tola·r·* as a verb, he conjectures that instead of *tola·r·* or *tola*, if the *r* is to be taken by itself, we should read *zona*, which he thinks might be broken off for *zona·s·to*.[14] *zona·s·to* is the commonest verb-form which we have preserved in the Venetic inscriptions and is interpreted as a 3rd sing. indicative middle of an *s*-aorist, equivalent in meaning to 'donavit'.[15] To justify this emendation Pauli remarks that in the Venetic alphabet *tola* and *zona* are similar enough. *t* in Venetic characters is a single cross and *z* a double-barred cross, and an *l* can be made into an *n* by the addition of a stroke. This reading would, however, leave the *r* hanging.

Sommer (IF 42.112), tentatively accepting Pauli's conjecture, makes a brilliant suggestion. He says: 'Hat Pauli 254, 278 recht, *zona·r·* zu lesen? An die Dedikationsformel denkt man ja unwillkürlich beim Vergleich mit 288 (287). Etwa gar, dem präteritalen *zonasto, zoto* mit 'medialem' *-to* sich beigesellend, eine 3. sg. p r a e s. medii auf *-r*, "Uenna donat *magiculum*"? Zum Praesens vgl. CIL. I, 28, 29 usw.' This suggestion, buried in small type in a note which mainly deals with other matters, has not, it seems to me, received the attention that it deserves. Conway makes no mention of it. I believe that Sommer was on the right track, only it appears to me unnecessary to accept Pauli's purely conjectural reading in order to avail ourselves of Sommer's acumen.

[10] See Pauli 208, and the examples there given.

[11] A good Greek parallel in IG 1.410.

[12] [The author here acknowledged certain suggestions of mine. As I have found it necessary to revise them, I shall reserve their publication for another issue. GMB]

[13] Cf. Conway, Outline of Venetic Grammar, Prae-Italic Dialects 1.185.

[14] 254; cf. 273–4 and Sommer 116–7.

[15] See Conway, op. cit., 1.190; Pauli 404; Thurneysen, WklPh 1892.289.

The reading of no. 169 is quite clear. It is regrettable, especially in the case of so important and interesting an inscription, that Conway did not succeed in seeing it himself (see Conway, Prae-Italic Dialects 1.170). Pauli however saw the inscription himself and confirms the reading of A. B. Meyer. The final -r of the second word is perfectly distinct both in Meyer's sketch and in Pauli's reproduction of his squeeze, and we cannot simply get rid of it. Pauli's suggestion that the r might be taken by itself, with what meaning he does not even hint, is merely due to his inability to interpret the puncts before and after the letter. These are, to my thinking convincingly, explained by Conway[16] as accent-signs. Before resorting to conjecture, we should, it seems to me, endeavour to interpret the text as the bronze actually has it. I should therefore read as the second of the three words of our inscription *tolar*.

If this reading is correct, and from an epigraphical point of view it seems reasonably certain, we have a most interesting and important new verb-form. *tolar* I consider morphologically the Venetic equivalent of Old Irish *canar*, *berar*, Umbrian *ferar*, etc. and, except for the personal ending, to be equated directly with Old Latin *tulam*. It would be associated with the dissyllabic heavy base *telā- 'lift up', 'carry', 'bring or bear.'

In regard to the vocalism of *tolar*, with the very limited knowledge that we have of Venetic phonology[17] I should certainly not wish to be dogmatic; but it appears to be normal. The long vowels are not distinguished from the short in the Venetic alphabet, but we may assume that the a of *tolar* was originally long, whether or not, in Venetic as in Latin, long a was eventually shortened before final r. From a dissyllabic heavy base we should expect either *nil*-grade or reduced-vowel grade in the first syllable in connection with long a in the second syllable.[18] We have then three possibilities, any one of which would give a satisfactory explanation of the vocalism of *tolar*:

[16] Op. cit. 1.191-7. Cf. Whatmough, ClPh. 29.283 (1934). The argument of this paper is, however, in no way affected if one does not accept Conway's theory of the accentual value of the puncts, provided that they are not interpreted in such a way as to separate the r from *tola*.

[17] Cf. Conway, op. cit. 1.197-9.

[18] Meillet, BSL 19.97, says that '*tlā est la forme à voyelle longue finale de la racine dissyllabique *telə.' It is—in Dor. ἔτλᾱν; but that is not necessarily the only form of the second full grade of the base. The reduced vowel also occurs in the first syllable of such bases (Hirt's Vollstufe II a, cf. Hirt, Indog. Gram. 2.121, 113; Ablaut 76, cf. 85).

(1) in Venetic *ₑl* gave *ol*, as in some early Germanic dialects. From this very base *tₑlā-* come OHG *dolon*, OSax. *tholon*, OEng. *þolian*, OFries. *tholia*, while Gothic *þulan*, etc. seem to represent a form of the base *tₑlē-*.[19] Hirt (Indog. Gram. 2.84) considers 'dass *or* [*ol*] die ursprüngliche Stufe war, da wir damit zu einer Annäherung an das sonstige *ar* gelangen.' That is, in certain dialects, of which Venetic may well have been one, the reduced vowel before *r* or *l*, like syllabic *ṛ*, *ḷ*, probably developed into *or*, *ol*. That Venetic shows striking resemblances to early Germanic in some points has been noted by Sommer (see ftn. 2).

(2) the word perhaps had an original *o*-coloring (cf. Lat. *tolero*, Gk. τόλμα, τόλμη) and *ₒl* developed normally in Venetic, as in Greek, under certain conditions at all events, into *ol*.[20]

(3) the original form of the word, i.e. in Primitive Venetic, was **tular*, corresponding strictly to OLat. *tulam*,[21] and later became *tolar* by analogy to a present similar to Latin *tollō* which we may assume existed also in Venetic. The existence of a Venetic present **tollō* or **tolnō* may reasonably be assumed, since Latin *tollō* is in all probability the thematic transformation of a *-nā-* present **tḷ-nā-mi*, and such *-nā-* presents generally are associated with *sēṭ-* bases.[22] Such a disturbance of original

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[19] See E. Kieckers, Handbuch der vergleichenden gotischen Grammatik 240 (Munich 1928).

[20] Cf. Walde, *O*-farbige Reduktionsvokale, Festschrift Streitberg 175–87 (Heidelberg 1924). Miss Ruth E. Moore of Montreal has very kindly communicated to me by letter a number of interesting examples in Latin and Greek (from a research that she has made in the reduced vowels) which she considers illustrations of *or*, *ol* developing out of the dull reduced vowel of *o*-color. E. g. Lat. *morior, mortuus* : Gk. μορτός·ἄνθρωπος, θνητός Hesych., βροτός; Lat. *orbus* : Gk. ὀρφανός, ὀρφεύς, ὀρφός; Lat. *orior* : Skt. *ṛṇóti*. It is of course extremely difficult, as both Walde and Miss Moore recognize, to determine whether in a given instance *or*, *ol* represent the reduced or the normal deflected grade in PIE.

[21] The *u* of Lat. *tulam* itself requires explaining, since *ₑl* normally gave *al* in Latin. Cf. Kent, Sounds of Latin §§80, 94. For varying views as to the origin of the *u* of *tulam*, see Brugmann, Grdr.² I.1.455–6; Meillet, BSL 19.97; cf. Reichelt, KZ 39.56; Hirt, Handbuch des Urgermanischen 1.59 (Heidelberg 1931). Meillet remarks: 'on ne recherchera pas ici si l'*u* de *tulam* est un ancien *u*, développé devant *l*, comme dans *gula*, cf. arm. *ekul* "il a avalé", ou si, ce qui est peut-être plus probable, *tulam* est tiré de formes à préverbe comme *attulam*.' This second view, which Meillet considers *plus probable*, would seem to imply that the earlier form of the Latin word, without prefix, was *tolam*, which would provide a perfect parallel for Venetic *tolar*. Cf. de Saussure, Mémoire 100: 'En latin la voyelle obscurcie en *u* pourra généralement passer pour *o*.'

[22] Cf. Walde-Pokorny, Vergl. Wörterb. der indog. Sprachen, s.v. *tel-*; Solmsen, KZ 38.445; de Saussure, Mémoire 239–42; Hirt, Indog. Gram. 2.109; Sommer, Hdb. der lat. Laut- und Formenlehre².³ 231.

ablaut relations through the action of analogy is of course an extremely common phenomenon.[23]

That Latin *tollō* did not exercise such an analogical influence on Latin *-tulam* is naturally accounted for by the fact that, in Latin, *-tulam*, as well as *(te)tulī* and *lātus*, was associated in a suppletive relation with *ferō*. *attulās* for instance would not be felt in popular etymology as coming from *tollō*, but from *(ad)ferō*. In Venetic it is very likely that such a suppletion of a verb corresponding to Lat. *ferō* by one corresponding to Lat. *tulī*, *lātus* did not exist. In Tocharian, for example, the verb *pär*, which corresponds to Skt. *bhárāmi*, Greek φέρω, etc., is completed by *kām*.[24]

We have no reason then to reject the reading *tolar* on phonological grounds if it yields a satisfactory sense.

The root *tel-* has a rich and varied semantic development in the various IE dialects, but its fundamental meaning appears to be 'lift up'. In Sanskrit *tulayati* it means '(lift up to) weigh', in Latin *tulī*, *lātus* '(lift up and) carry, bear', in Latin *tollō* 'lift up and take away', in Greek ἀνατέλλειν 'lift up (one's self)', 'rise' (of stars). Other meanings, such as 'endure', seem to be secondary. In Venetic I conceive that it meant '(lift up and) dedicate', corresponding in general significance to the Greek ἀνέθηκε so common in dedicatory inscriptions of all kinds. From the same root the Greek noun τέλος in the plural is used in the sense of 'offerings', 'sacred rites'.

Though the meaning 'dedicate' seems to me the most appropriate and probable signification of *tolar* as it occurs in our inscription, on a bronze plate which appears to have been fastened on a votive object,[25] it is also possible to take it as meaning, 'carry', 'bring' (an offering), corresponding then in signification to Latin *tulī*, *-tulam*. Compare CIL 1² 34: PL. SPECIOS | MENERVAI | DONOM | PORT (editor's note: PORT videtur esse *port* [at]).

For the use of the present tense in a dedicatory inscription, besides the archaic Latin inscriptions cited by Sommer (CIL I² 28, 29, etc.), we may compare the fact that in Boeotian Greek, for example, ἀντίθειτι is always used in manumission inscriptions, which have regularly the

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[23] Cf. Hirt, Indog. Gram. 2.192; Carnoy, Manuel de linguistique grecque 164 (Louvain 1924).

[24] See E. Fraenkel, IF 50.226 (1932); Schulze, Sieg, Siegling, Tocharische Grammatik 448 (Göttingen 1931). Cf. Osthoff, Vom Suppletivwesen 4, 6.

[25] In no. 166, with which no. 169 according to Conway ad loc. is to be classed, the verb *zonasto*, commonly used in dedicatory inscriptions, is preserved.

form of the dedication of the slave to a deity.[26] δίδωτι is also found in
the same dialect inscriptions parallel with ἔδωκε.[27] What we have in
tolar would be an aoristic or 'punctual' use of the present tense and this
would be strictly appropriate to the form of the word. If Conway is
right in his interpretation of the double puncts in Venetic as having an
accentual value, then the puncts occurring in this instance before and
after the final *r* of *tolar* would indicate that the word was accented on
the second syllable. As Venetic was conservative in its system of
vowel sounds, so far as we can judge from its scanty remains, so it ap-
pears to have retained the original accent of this dissyllabic base on the
second syllable[28] and this goes along with an aoristic meaning.[29]

If we are on the right track in the etymology and semantics of *tolar*,
its meaning here, on a dedicatory bronze,[30] can hardly be other than
indicative and personal, 'Venna dedicates'. It is also clearly middle,
and not passive. The characteristic nuance of meaning of the middle
voice, expressing the personal interest of the worshipper in his votive
offering, occurs constantly in Venetic inscriptions in the verbs *zonasto*
and *zoto*.[31] Conway remarks (Outline of Venetic Grammar, op. cit.
1.201, §64) that 'the verbal forms showing what in Greek would be
called Middle endings (*z·o·to, zona·s·to*) show no perceptible difference
of meaning from those of the Active.' But the middle voice often shows
no difference in meaning that is perceptible to the Anglo-Saxon mind,
yet the difference is there, would we observingly distil it out. The
worshipper gave, hoping to obtain *for himself* the favor of the God.
That the middle voice was in living use in Venetic appears also in all
probability in the interesting verb-form *vhaχ·s·θo*, (no. 150) plausibly
explained as meaning 'fecit'. Here too Conway seems a little insensi-
tive to the expressively 'middle' significance of the form. It is not
necessary to suppose that it meant 'just conceivably ἐποιήσατο, 'fieri

[26] See my Syntax of the Boeotian Dialect Inscriptions 68.
[27] Ibid 69.
[28] See Conway, op. cit. 1.197, §50; and 194, §41.
[29] Cf. Carnoy, op. cit. 165. Meillet (BSL 19.95 ff.) is inclined to consider this
ā-formation as autonomous, but not necessarily aorist in origin. The Venetic
form would tend to confirm its aoristic character, as recognized by most scholars
(see, e.g., Stolz-Leumann, Lateinische grammatik 325 and the references there
given). Cf. Vendryes, Sur quelques présents en -Ā́- du verbe italo-celtique,
MSL 16.305: 'On aboutirait donc à reconnaître en latin trois groupes de présents
en -*ā*- issus d'anciens aoristes.'
[30] For the probably votive character of these bronzes, see Conway, op. cit.
1.171–2, cf. 173.
[31] See Conway's Resultant Text, op. cit. 1.6–18 *passim*.

iussit', an insc. of the owner or donor'. It is sufficient to assume that it indicated the natural personal interest of the subject in his own artistic creation.[32]

That the medial ending -*r* (without *t*) should occur in Venetic in association with the middle ending -*to* is of great interest. One of the stumbling-blocks in the way of a more unreserved recognition of the middle character of the verbal ending -*r* has been no doubt its complete absence in Greek. Since Venetic represents, as Conway says, 'a type of inflexion in many ways intermediate to that of Greek and Latin', it seems to me not improbable that the medio-passive *r* once existed in Greek also and was later lost. Venetic would thus show the transition stage between Greek with its -*to* endings and Latin with its -*r* endings (some of which contain also the element -*to*-) and would be more archaic in this respect than either of them. Such a total disappearance of a once highly-important characteristic is not unknown. In the Northwest-Caucasian dialects, for example, the class-signs, so characteristic of the certainly-related Northeast-Caucasian languages, and presumably of Primitive Caucasian, have completely disappeared, except in Abkhaz (which has only a very few).[33]

An interesting light is thus thrown by the Venetic verb that we have been discussing on the Old Irish verb of kindred formation, *canar*. *canar* has been cited as meaning 'there shall be singing'.[34] Yet in the Old Irish documents *canar*, so far as I am aware, always means 'is sung'. It occurs several times in the Tract on the Mass in the Stowe Missal, and is there rendered by Stokes and Strachan 'is chanted',—for example: Huisq*ue* p*rius in* calicem iss*ed* canar occo, 'Water, first, *in calicem*, and this is chanted thereat.'[35] *Canar* is here obviously parallel in meaning to the Latin pres. ind. pass. *canitur*.[36] I am inclined to think, therefore,

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[32] Sommer, also, IF 42.126, takes *vhaχ·s·θo* as 'eine Meistersignatur' and transcribes *faχsθo* = *fak-s-to*, though without commenting on the meaning of the middle ending.

[33] See G. Dumézil, Études comparatives sur les langues caucasiennes du nord-ouest 105, 117–8 (Paris 1932), but cf. Id., Introduction à la grammaire comparée des langues caucasiennes du nord 32–7 (in Bibliothèque de l' Institut français de Léningrad, Tome XIV, Paris 1933); A. Dirr, Einführung in das Studium der kaukasischen Sprachen 39, 342–3 (Leipzig 1928).

[34] W. Petersen, AJP 53.209 (1932); E. H. Sturtevant, Comparative Grammar of the Hittite Language 262 (1933).

[35] Thesaurus Palaeohibernicus, edited by Whitley Stokes and John Strachan, 2.252, 1.5; cf. ibid. 252, ll.12, 14, 19, (Cambridge, 1903).

[36] Ibid. 253, 1.3 and 1.5; compare the use of *arcanar* in Glosses on the Carlsruhe Beda, Thesaur. Palaeohib. 2.18, 1.33, where it glosses Latin *cantatur*.

that the rendering 'there shall be singing' is merely a translation of Brugmann's 'man singe' (Grdr.[2] 2.3.665). But Brugmann is here simply theorizing,[37] more or less under the influence of Zimmer's 'man'-form hypothesis of the verbal *r*-endings, a hypothesis of which I have elsewhere,[38] I believe, shown the untenability. In the absence of any positive evidence that *canar* really means 'there shall be singing', it cannot convincingly, it seems to me, be used to bolster up the theory of a volitive impersonal in *-r*.

In reality, the comparison of Old Irish, Latin, Oscan, Venetic, Greek, Lithuanian, Old Ch. Slav., and Tocharian appears to point to the preponderantly indicative character of this whole *ā*-formation. Not only are O.Ir. *canar*, *berar*, etc., conjunct forms of the present indicative,[39] occasionally functioning as imperatives, but also Ir. *ba* is preterite indicative as well as present subjunctive. In Latin beside the present subjunctives in *-ā-* we have the imperfect indicatives in *-bam* and also *eram*, the latter perhaps inherited from the parent speech.[40] There is, too, Latin *inquam*. Whatever our theories as to the origin of this word, the fact is that as it actually occurs in Latin literature it is not only indicative, but strongly indicative, as is shown both by its ordinary use in introducing a direct quotation—the speaker's *ipsissima verba*—, like English 'said I', 'say I', but especially by its idiomatic employment 'after a word which the speaker strongly emphasizes, especially in repe-

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[37] Cf. 'Vermutlich' 664 *ad fin*. . . . 'Es mag hiernach' . . . 'vielleicht also' 666. Brugmann here seems to be reproducing the argument of Thurneysen, KZ 37.104. On page 705 also Brugmann renders Ir. *canar* by 'canatur', but this too is purely a translation ad argumentum, since, as we have seen, the Irish themselves use *canar* to gloss Lat. *canitur* and *cantatur*, both indicatives.

[38] The Hypothesis of the Italo-Celtic Impersonal Passive in *-r*, LANGUAGE 5.232–50 (1929).

[39] Cf. R. Thurneysen, KZ 37.104: *Berar fedar dlegar*, die Gr. Celt.[2] 474 als conjunctive aufgeführt werden, sind alle drei indicative; bei den letzteren geht das schon daraus hervor, dass diese verba nur den *s*-conjunctiv bilden.

I am of course aware that Thurneysen, Hdb. des Alt-Irischen 1.345, says: 'Die mangelnde Synkope erweist, dass sich -*ar* zunächst aus silbebildendem -*ṛ* entwickelt hat (§109), z. B. ·*canar* aus *·*canṛ*.' This would seem to remove *canar* altogether from the class of *ā*-forms. I agree with Pedersen, however (Kelt. gram. 2.402, ftn. 1), that this assumption is 'keineswegs notwendig'. It seems highly improbable that we should separate Ir. *canar* from Umbrian *ferar* or *ferar* in turn from Lat. *ferātur*, which closely glosses it.

[40] Cf. M. Leumann, Die ital. *f*- und *b*-Tempora, IF 42.70, ftn. 1: 'nur halte ich es [*es-ām*] nicht für eine junge, sondern für eine uralte, vielleicht schon idg. Nachbildung von *bhuय्-ām* (vgl. lit. *bùvo*).'

titions'.[41] So Lucretius is nowhere more passionately asseverative
than in the famous passage (2.256–8) where he is asserting the freedom
of the will:

> libera per terras unde haec animantibus exstat,
>
> unde est haec, inquam, fatis avolsa voluntas,
>
> per quam progredimur quo ducit quemque voluptas.[42]

The same tone of positive affirmation meets us in Cicero (Verres 2.4.29.
§67): 'rex maximo conventu Syracusis in foro, . . . in foro, inquam, Syra-
cusis.' In view of this inherently affirmative character of *inquam* in
the literature it appears probable that it, too, was originally a past
indicative.[43] The transition from 'said I' to 'say I' is extremely easy
and natural. The testimony of *inquam* is noteworthy, since it is es-
pecially from such isolated and peculiar archaic forms that we may
expect to get light on obscure questions of origin.

In Oscan, **fufans,** whether or not it was originally a pluperfect,[44] is
certainly indicative. In Venetic, *tolar,* as we have seen, is clearly
indicative and in Greek the kindred ἔτλᾱν is nothing but indicative.
Lithuanian *bùvo,* though parallel with the Latin subjunctive *fuat,* is
probably a more archaic form and is purely indicative.[45] The evidence
of Lithuanian is of especial importance because of the generally archaic
character of that language.[46]

The Old Ch. Slav. iteratives in -*a*- and -*va*- seem to be a kindred
formation, as is indicated by the fact that if ablaut occurs in the forms
of the primary verb, the iterative shows the weakest vowel-grade.[47]
And they function as indicatives.

The third plural *bǫ,* equivalent morphologically to Latin *fuant* and
used with conditional force, might seem to point in the other direction.

[41] Harpers' Latin Dictionary, s.v. Cf. G. Lodge, Lexicon Plautinum, s.v.
Inquam—1. *iterat atque confirmat*: . . . 2. *orationem rectam affert*. (1904–1924).

[42] Cf. W. A. Merrill on Lucretius 3.341: '**inquam** L. writes when he is very much
in earnest.' (T. Lucreti Cari, De rerum natura, Libri sex, New York, etc., 1907).

[43] Cf. Stolz-Leumann, Lateinische Grammatik 325: '*inquam* (falls ursprüng-
lich Praeteritum).' This seems more likely than that it was 'ein eingeschaltetes
"möcht' ich sagen" ' (Sommer, Hdb. 488, cf. Brugmann, Grdr.² 2.3.540, 541, 867).

[44] See W. Petersen, The Italic Imperfect, Lang. 3.175–83 (cf. Lang. 8.133–7);
and, on the antiquity of the form, cf. M. Leumann, IF 42.69, ftn., and 72.

[45] Cf. M. Leumann, IF 42.72: '*fuam* "ich sei" ist lat. Neubildung.'

[46] Cf. H. Koppelmann, Die Eurasische Sprachfamilie (Heidelberg 1934), Vor-
wort.

[47] See A. Leskien, Handbuch der altbulgarischen Sprache⁶ 18 (Heidelberg
1922).

But since the use of this form in the Old Ch. Slav. sources is chiefly as an expression for the unfulfilled condition, while its employment in a wider modal sense became more extended later,[48] *bǫ* appears to be no absolute bar to the view that in Old Ch. Slav., also, the original force of the verbal forms with the element -*ā*- was indicative. In Greek, too, the unreal condition is expressed by the indicative mood.

In Tocharian, relations are somewhat obscured by the fact that in that language the vowel *ă* may represent original *ē* as well as original *ā*. It appears probable, however, that Tocharian, too, had an *ā*-subjunctive, which was often indistinguishable from a preterite indicative except by the personal endings.[49]

Venetic *tolar* and its congeners thus tend to confirm Hirt's early scepticism as to the existence of the Injunctive.[50] Even in Sanskrit, where the Injunctive may be said to be especially at home, the non-modal use of the augmentless verb-forms still slightly predominates in the earliest period of the language.[51]

The hypotheses of Brugmann (Gr.[2] 2.3.539–40) and Thurneysen (KZ 37.106), designed to explain a supposedly original volitive character of verb-forms such as *abstulās* and *canar*, seem, therefore, as unnecessary as they are, to my thinking, improbable. As regards Thurneysen's derivation of Ir. *canar* and similar forms from an Italo-Celtic infinitive-form, used to express a command, I have already pointed out (LANG. 5.246) the inconsistency of the reasoning by which he arrives at this result. And Brugmann's idea that the whole subjunctive in -*ā*- may have developed from the accusative case of an abstract noun, such as Skt. *tulā́*, seems equally unconvincing. I have difficulty in imagining even a primitive Indo-European (if I may use the term), or Italo-Celt, saying to himself 'weighing!'—with a passionate accusative—and then proceeding to develop a subjunctive mood from a confusion of this accusative with the first person singular of a verb.

[48] Id., op. cit. 137.

[49] See Carl J. S. Marstrander, Norsk Tidsskrift for Sprogvidenskap 3.243 (1929), and the references there given; W. Schulze, E. Sieg, W. Siegling, Tocharische Grammatik 325, 341 (Göttingen 1931). Cf. A. Meillet, Bull. 19.98, 'L'emploi des désinences secondaires ne prouve donc rien'; Id., Esquisse d' une histoire de la langue latine[2] 25 (Paris 1931).

[50] See H. Hirt, IF 12.212 (1901): 'Das hat mich an der Existenz des Injunktivs immer ein bisschen zweifeln lassen', and compare his recent view, Handbuch des Urgermanischen 2.133 (1932). Cf. Meillet, BSL 19.98.

[51] See John Avery, Unaugmented Verb-Forms in the Veda, JAOS 11.330–1 (1885), and the statistics there given.

In view of all the evidence, the theory of Delbrück[52] that the Injunctive is not an independent mood, but an offshoot of the Indicative, seems still the most probable.

SUMMARY

Venetic *tolar*, Old Irish *canar* (in both its imperative and its indicative uses) and the 'Injunctive' are alike interesting examples of the rich potentialities of development inherent in the original Indo-European indicative mood. What nuance of meaning may once have been imparted by the element -$\bar{a}$- it is perhaps now impossible to determine.[53]

[52] Vergleichende Syntax der indogermanischen Sprachen, Grundriss 4 (1897). 356.

[53] Cf. Hirt's remark (Indog. Gram. 2.113), 'dass das Suffix -$\bar{a}$ eine vollständig unklare Grösse ist.' But see Meillet, BSL 19.98, and Vendryes, MSL 16.300–5.

Uomo AS AN INDETERMINATE PRONOUN

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After serving for a few centuries in the several Romanic languages as an indeterminate pronoun, *homo* disappeared from general use in all except French. The present study, in supplementing the limited discussion of grammarians,[1] briefly reviews the history and the use of this pronoun in Italian and deals mainly with the causes that led to its desuetude.

To account for the rise of the pronominal function of *uomo*, Meyer-Lübke suggests French influence.[2] But the general occurrence in Romance of *homo* with the value of an indefinite pronoun points rather conclusively to an antecedent development in Latin. Examination of the proper Latin authors reveals that *homo* was actually employed with pronominal force,[3] and there is consequently little necessity of attributing the rise of this use of *uomo* to foreign influence.

Uomo (< *homo*), like French *on*, was in origin a masculine substantive in the nominative case.   We shall see to what extent these characteristic traits were preserved.

According to P. Petròcchi, the vogue of *uomo* as a pronoun was

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[1] There has recently appeared a scholarly dissertation by Rita Schlaepfer, Die Ausdrucksformen für "man" im Italienischen (Zürich, 1933), which treats *uomo* and its synonymous expressions from the historical, descriptive, and geographical points of view.

[2] Gram. des langues romanes 3.108 (New York, 1923).

[3] Cf. Silviae vel potius Aetheriae peregrinatio ad loca sancta,[2] ed. W. Heraeus (Heidelberg, 1921):

    ubi homo desiderium suum compleri videt, 13.1; 36.3;

Vulgate, Matt. 4.4:

    Non in solo pane vivit homo;

Kleine Texte zum Alexanderroman, ed. F. Phister (Heidelberg, 1910):

    ubi habet homo prosperitatem, 24.12; 8.32; 10.24;

Disciplina Clericalis des Petrus Alfonsi, ed. A. Hilka u. W. Söderhjelm (Heidelberg, 1911):

    quo modo debet homo comedere coram rege, 40.13.

Grammarians cite many other examples.

limited to the thirteenth, fourteenth, and fifteenth centuries.[4]   We find many instances of its use subsequent to this period but only one prior, in a twelfth century Gallo-Italic text included by E. Monaci in his *Crestomazia* :[5]

> *or la rei, quant hom la met en l'aiva,* 12.

As with its counterparts in other Romance languages, there was hesitancy over the use or omission of the definite article with *uomo* :

> *merzé com omo clama,* Monaci 46.
> *fere in tal loco che l'omo non spera,* Ibid. 54.
> *e zo vol dire hom che sia amore,* Ibid. 59.
> *Potrebbe già l'uomo opporre contra me,* Vita nuova 12.17.
> *da ciò è l'uomo rimosso,* Convivio 1.2.3.
> *ke veniamo de la montangna*
> *ke ll'uomo apella Mongibello,* Monaci 449.

Inversely, *uomo*, anarthrous, sometimes conveyed the sense of the generic:

> *aisì est fait omen cum est la rei del pescaor,* Monaci 12.
> *anno, dine et ora omo non po sperare,* Monaci 476.
> *Puote uomo avere in sè man violenta,* Inferno 11.40.
> *Opera naturale è ch'uom favella,* Paradiso 26.130.

This anomaly has various possible explanations: it may have been due to the action of the sense of euphony as perhaps in French; to the looseness which prevailed in the use of the article at the time; to the natural inconsistency of poetry, almost the sole literary form upon which we must depend for the first century; and, especially, to the substantive origin of the term and to the innate confusion between the indefinite and generic ideas.

No confusion existed, of course, when *uomo* referred to an indefinite unity (= *unus, aliquis, quisquam*) or to a small plurality, and none arose when the reference was to man in distinction from God, Nature, Death, Love, woman, and the like, for then the sense was predominantly generic.   In other instances, the two constructions parallel each other in making no attempt to individualize and in selecting a noun in the singular number to convey their respective ideas.   It was in the operation of their inherent functions that they approached synonymity: the generic, in referring to the whole of the group and in failing to individualize, is actually indefinite, and, conversely, the indefinite, when embracing a large plurality, approached, if it did not merge into, the generic.   Because of its approximation to the sense of

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[4] Novo dizionario universale (Milano, 1917–19), Vol. 2, *s.v.*
[5] Italiana dei primi secoli (Città di Castello, 1912).

the generic, the indefinite construction was preceded by the definite article; in other cases, the inability or neglect to determine the size of the plurality or whether or not the idea was indefinite or generic resulted in a vacillation over the use or omission of the article.

In the following classification of the uses of *uomo*,[6] it will be noted that especially in the oblique cases the sense approaches the generic:

    I. Normal

        vole homo dicere, Monaci 130;

        ke veniamo de la montangna

        ke ll'uomo apella Mongibello, Ibid. 449;

        fae sì grande laude fare

        cket homo no lo potera estimare, Ibid. 541;

        non direi dover uomo arguire, S. Speroni 39.[7]

    II. Qualified by

        A. A relative clause[8]

            1. Nominative

                Tenea, com'uom che reverente vada, Inferno 15.45;

                Allor mi volsi como l'om cui tarda, Ibid. 21.25;

                . . . . e non è da blasmare

                omo ke cad'en mare, se s'oprende, Monaci 53;

            2. Oblique

                a. Object of preposition

                    Fami tener manera d'omo k'è'n disperança, Monaci 63;

                    no dex a l'hom ke mangia, Ibid. 402;

                b. Accusative

                    k'io non agio veduto

                    omo k'en gioja mi possa aparilgliare, Ibid. 85;

            3. Negative (usually = *nessuno*) or semi-negative

                che non è om che la veggia, Ibid. 516;

                non avesse huomo ke potesse sedere, Cento novelle ant. 64;[9]

        B. A prepositional phrase

            c'omo di grande affare perde lo suo savere, Monaci 66;

        C. An adjective or present participle

            Sicom omo improdito, lo cor mi fa sentire, Ibid. 52;

            Disse'l maestro, ansando com'uom lasso, Inf. 34.83.

[6] Schlaepfer classifies the uses of *uomo* in three groups: Negations, comparisons, and conditions.

[7] Dialogo delle lingue, ed. G. de Robertis (Lanciano, 1912).

[8] *Uomo* may still be found today in this construction; it does not appear to be a bona-fide indefinite.

[9] Ed. E. Sicardi (Strasbourg, s.d.).

III. In an oblique case

    A. Object of preposition

        amor, ch'en omo asende poi li piace, Monaci 66;

    B. Accusative

        qu'el conduçe l'omo tosto a desenore, Ibid. 115;

        El basalischio serpente occide om col vedire, Ibid. 472.

IV. *Homo canta = cantiamo*.[10]

For the use of *uomo* in the early centuries it is superfluous to cite examples; a few from the sixteenth will suffice to indicate this term still remained in that period:

    se l'uomo vuol comprar panno o scarpe, vuol che di buona lana o di buon coio siano,   M. Bandello, Le novelle 58.[11]

    si partì de lei carico di tante busse, quante mai uomo potesse portare,

                      G. F. Straparola, Piacevoli notti 96.[12]

    che siete lasciato stare per ischifezza, per indegno che l'uomo vi guardi,

                           A. Caro, Opere 138.[13]

    la gotata, che l'uomo dona a novello cavaliere,

                      A. Doni, Tutte le novelle 98.[14]

The latest prose examples of *uomo* cited by Schlaepfer[15] are from Tasso (†1595):

    se ben l'uomo è incerto de l'ultima, Dialoghi 2.68;

    e che l'uomo sottoponga il suo parere, Ibid. 3.65.

It remained in use, however, for a few more years:

    quando l'uomo crede averli compresi in un indrizzo, scappano,

               P. Sarpi, Lettere ai protestanti (1609) 79.[16]

    né può uomo sentirla senza ridere,

               T. Campanella (†1639), Lettere 55.[17]

For poetry, Schlaepfer's latest example[18] is from G. Chiabrera (†1638):

    Non fu Dea sua genitrice,

    com'uom dice, Bellezza cagione di tormento in Magg. 4.170,

[10] This use of *uomo* has been treated at length by W. Meyer-Lübke, Ital. Gram. 222 (Leipzig, 1890); and Bertoni, Flecchia, Salvioni, et al.

[11] Ed. G. Brognoligo (Bari, 1910), Vol. 1.

[12] Ed. G. Rua (Bari, 1927), Vol. 1.

[13] Ed. V. Turri (Bari, 1912), Vol. 1.

[14] Ed. C. Teoli (Milano, 1863).

[15] P. 58.

[16] Ed. M. D. Busnelli (Bari, 1931), Vol. 1.

[17] Ed. V. Spampanato (Bari, 1927). Also: *I parenti s'hanno quali la ventura gli dà, gl'amici quali l'huomo se li sa scerre*, Letter of A. Sertini to Galileo, in Le opere di G. Galilei, Edizione Nazionale 10.411 (Firenze, 1900).

[18] P. 58.

yet subsequent examples can be found:

> *Qual uom talora in alta notte suole,*
>
> > G. F. Maia Materdona, Lo sdegno
> > liberatore, published 1632.[19]

> *venne uom giamai d'in voi penar sí degno,*
>
> > G. Manso (†1645), La solfatara, publ. 1640.[20]

> *Tua mente in mirar l'uom,*
>
> > A. Basso, A Frate Angelo, publ. 1645.[21]

We may therefore cite the middle of the Seicento as the *terminus ad quem* for the pronominal use of *uomo*. The archaic nuance of the latest examples is of course evident and natural. Since the course of a construction seldom comes to a full stop, it is to be expected that examples subsequent to this date will appear in poetry, proverbs, or in other mediums in which the archaic is employed.

The phase in the history of the present construction the most fascinating is perhaps that offered by its disappearance. Language as a living organism is continually in a state of flux; in its growth and development its resiliency and flexibility are attested by countless instances of the introduction of new elements and of the discard of those which have become outworn, out-moded, inadequate, or superfluous.

The failure of such an apparently serviceable and valuable construction as the indefinite pronoun *uomo* to survive presents a linguistic enigma. While we do not presume to solve the inexplicable, there were in evidence forces of a tangible nature and of sufficient influence to consign the pronominal use of *uomo* to the limbo of forgotten usage; to these we shall now turn.

While it is true that the majority of texts in the first centuries of Italian contain examples of *uomo*, examination reveals that bona-fide examples, that is, those which are exactly equivalent to French *on*,[22] were comparatively infrequent. In 700 pages of the Decameron, for instance, only eight examples appear, and six of these are preceded by the definite article. Since in linguistics as in physiology a requisite for preservation is exercise or use, the infrequent demand for this par-

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[19] In Lirici Marinisti 108, ed. B. Croce (Bari, 1910).

[20] Ibid. 34.

[21] Ibid. 337.

[22] Cf. 'Hat *uomo* nur eine sich dem unbestimmten Pronomen annähernde Bedeutung', L. G. Blanc, Gramm. d. ital Sprache 310, Anm. (Halle, 1884). Also, '*uomo* presso a poco = *on*', G. A. Scartazzini, Enciclopedia dantesca (Milano, 1896–1905), Vol. 2, *s.v.*

ticular mode of expressing the sense of the indefinite subject was in part instrumental in effecting its desuetude.

This lack of popularity can be explained by the fact that there came to be felt a certain inadequacy in the use of *uomo* as an indeterminate pronoun and hence discontent with its use. In what did this inadequacy consist that aided in expelling it from the language, and how did it arise? A word often contains the germ of its own destruction; probing the capacity of the present term we soon detect its limitations as an indefinite. The indefinite subject construction par excellence is one that has lost its literal meaning; compare the indefinite reflexives, Spanish *se* and Italian *si*, which in becoming logical subjects have lost all trace of their original reflexive value, and also French *on*, which for very few has any etymological relationship with either *homme* or *l'homme*, because of their pedantic orthography. But contrast the fate of *uomo*: through an unfortunate phonological development, phonetic or syntactic doublets failed to evolve in Italian from *homo*, *hominem* as in French, that is, a separate and distinct form for the indefinite and another for the other uses of the term. The resultant acoustic equivalence, or homonymity, naturally though paradoxically provided *uomo*, indefinite, with a more or less definite connotation. It was difficult to dissociate from the term when used indefinitely its definite and primitive meaning, and this retention of its literal value hampered its use as an indefinite. Further, the homonymity, as may be expected, exerted its occasional eliminating effect upon *uomo* as an indefinite; as often happens, the less used term disappeared rather than its better known, more frequently used homonym.

It naturally follows that the greater the indefiniteness or vagueness of indefinite subjects, the greater the possibility of their success. In fact, the main distinction, it would seem, between *uomo* and *on*, and upon which their failure or success was in great part predicated, lies in the degree of indefiniteness respectively conveyed. The high degree of abstraction of *on* is well known; and this degree, it is important to note, *uomo* often failed to reach; cf.:

dolcie è lo male ond'omo aspetta bene, Monaci 78.
non vale c'omo ti (la morte) possa fare, Ibid. 96.
sapemo c'omo non trova maggiore,
se non singnore Dio, Ibid. 226.

This failure to maintain a uniform degree of abstractness constituted another eliminative factor.

The failure of *uomo* always to divest itself conclusively of its literal

meaning and to achieve this uniform degree of indefiniteness argues a state of arrested development.   Had it been permitted to evolve conclusively and distinctively in its new meaning, the possibility of its retention would have been much greater.   Contrast, on the other hand, the development of French *personne*: beset with fewer and less effective competitors, it was allowed to evolve conclusively as a pronoun; in an unrestricted development, it not only lost, like *on*, its etymological gender and substantive value, but achieved an entirely new meaning, one in fact diametrically different, exclusively negative (or interrogative), and this insured its retention in the language.

We have seen that *uomo* designated a subject that was less abstract, less indefinite, than that with which we usually associate *on*.   Such values had a deterrent effect not only upon its use as an indeterminate pronoun, but upon other possible uses as well.   With these limitations and obtrusive elements, it is evident that it could not serve as freely as it otherwise would have done as a substitute for the vague and shallow passive voice, a function that is one of the two chief raisons d'être of an indefinite subject construction and a *cheville* for its retention. Hence we see that *uomo* was more or less limited to the function of designating an indeterminate subject, with the resultant weakening of its already precarious hold.

But even in this function the inherent characteristics of the term continued to obtrude to prevent it from attaining the free scope and varied application that would assure its continuous use in the language.   In referring almost exclusively to the third person and to a fairly large plurality, and in retaining its masculine gender and singular number, *uomo* failed to concur in the usage of its counterparts, *on*, *se*, and *si*, either in substituting for the various subject personal pronouns of all persons and both numbers, or in designating by implication one single individual, as do the pronouns in *on frappe* and *si bussa*, 'one (person, someone) is knocking'.   Nor was there possible any such development as that of *on* in the sylleptical *on est belle; on est égaux; on se marie, nous deux; tous (nous) on va*, etc.   Hence in retaining its original accidence, *uomo* considerably circumscribed its application as an indefinite and lent itself more readily to dislodgment.

Parallel constructions: besides homonymity, infrequent use, and limited application, another important factor to be considered in determining causes which lead to the abandonment of elements from language is the existence of parallel or synonymous terms with their eliminating coaction, for in linguistic history it is axiomatic that a synonym

has an eliminating effect upon its companion.   Language abhors super-
fluity; it takes measures not against the rise of superfluities but against
their continuance.   Below are listed the various modes of conveying
more or less the sense of an indeterminate subject in Italian; it is to be
noted that they appeared in the language simultaneously with *uomo*
and that consequently from the very beginning the competition they
set up jeopardized the existence and development of *uomo* as an in-
definite:

la gente	*chi*	the indefinite and second and
molti	*nessuno*	third person singular (*dice*)
alcuno	*un uomo*	and plural (*dicono*).[23]
quello (*che*)	*uno*	

The passive voice: concerning the competition offered by the passive
voice, little need be said.   In Latin it was the normal mode of expressing
the sense of an indeterminate subject, and in Italian, now formed ana-
lytically, it continued to enjoy, despite its limitations, greater popu-
larity in this function than did *uomo*.   Of greater popularity likewise
was the new, reflexive passive.   The competition of the passive voice,
whether formed by the verb 'to be' and a past participle or by the third
person of the reflexive, was doubly effective in that, like other terms in
this list, it competed with both functions of *uomo*, that of expressing the
idea of an indeterminate subject and that of expressing the passive
voice.

The indefinite reflexive: of the various Latin constructions that
passed into Romance, the reflexive has perhaps attained the highest
degree of development, particularly in Italian where uses not dreamed
of in the mother tongue came into existence.   Once this construction,
so hospitably received, was happily seized upon to render the sense of
both passive voice and indeterminate subject, the fate of *uomo* with
these functions was sealed.   Again we have rivalry from the be-
ginning of Italian, for, like *uomo*, the indefinite reflexive, often undis-
tinguishable from the passive reflexive, dates back to the pre-Romance
period.[24]   This construction with its infinite capacity[25] we consider to

[23]Also, *il mondo, il cuore, la penna; manti; qualcheduno, qualcuno; altri; colui,
coloro, quei* (*che*); *neuno, niuno, nullo; gli uomini; ognuno;* etc. and the infinitive
and present participle constructions.

[24] *Littera se scribit* is cited by C. H. Grandgent, Introduction to Vulgar Latin
52 (Boston, 1907).

[25] The indefinite reflexive, it will be recalled, may be construed personally
with a thing, person, or clause as its subject; impersonally and intransitively
alone, with an adjective, adverb, present participle, or prepositional phrase;
and impersonally and transitively with a person or thing as the object.

have been the principal competitor of *uomo* and the chief instrument in effecting its downfall.

In discussions of the disappearance of constructions from language, such questions often arise: 'Did the old term die out of itself?' 'Did a new construction kill the old?' 'Were there reciprocal influences or actions?' 'Why did one of two synonymous terms survive while the other fell?'

The limitations of *uomo* were indeed sufficient for us to assume that the term died out of itself. This effacement, however, cannot but have been aided and hastened by the existence, as we have seen, of synonymous expressions. Whatever coup de grâce was necessary, the indefinite reflexive was at hand to deal it. The inadequacy of *uomo* in turn served to enhance the value of *si*.

As for the rivalry of *uomo* with the indefinite reflexive and other constructions, we have seen that the reflexive passive as well as many others appeared in the pre-Romance period; with the breakdown of cases, the reflexive in the type *se scribit littera* was doubtless felt to be the logical subject; consequently the question of precedence in Italian (*i.e.* the 'new' or 'old' construction) does not arise. Once in the language, the indefinite reflexive developed so rapidly that by the time we reach the Commedia, it was as frequent, if not more so, than *uomo*, a degree of popularity which it has maintained and increased down to the present day.

If we search for logical accounts of the processes by which preference is given to this one of two synonyms rather than to that, we are almost invariably disappointed. The indefinite reflexive, however, possesses various evident qualifications which equip it to serve as an indeterminate subject par excellence and favor its use to the exclusion of *uomo*. As we have indicated earlier, a term best serves as an indefinite subject whose literal meaning has become obscured or forgotten. This is what happened to *si*, for in assuming the rôle of an indefinite, it lost all trace of its original reflexive value. *Uomo*, on the other hand, as we have seen, was not successful always in divesting itself of its original force. As in the development of organic nature, the greater or lesser fitness of the forms which arise is decisive for their survival or disappearance. The malleability and flexibility of *si* as compared with the cumbrous, dissyllabic *uomo* need not be dwelt upon.[26] Economy of effort and the

[26] While it is true that monosyllabic forms *om*, *on*, and *uom* appeared, ending in a consonant they must have been felt too aberrant for general acceptance or for retention in the language long enough for further phonological development, if possible, more in consonance with the orthography of the language.

tendency to ease and abbreviation militated in favor of the reflexive. As the need for exactness in communication increased, the fact that the reflexive expressed unequivocally the sense of an indeterminate subject added to its attractiveness. Suppression of such terms as *uomo* is not really a loss; on the contrary, the language gains thereby in rapidity and in energy.

To sum up, *homo* appeared sporadically in the pre-Romance period of Latin as an indeterminate pronoun, a function to which *uomo* naturally fell heir. As late as the middle of the seventeenth century we find instances of *uomo* so employed, particularly in the nominative case. The effacement of this special use of *uomo* was not a sudden phenomenon but rather the result of the cumulative effect during several centuries of various causes which may be divided into two groups: (1) Those arising from the failure of two distinct forms to evolve phonologically from *homo*, *hominem*, as *on* and *homme* in French, *i.e.*, one form for the indefinite use of the term and a separate one for its other functions. Various limitations thus attached to *uomo*, such as the persistent retention of its original substantive value and a certain degree of definiteness, both of which naturally circumscribed its function as an indefinite. (2) Those arising from the existence of an array of substitutes or synonymous expressions, chief among which was the indefinite reflexive construction with its manifold ramifications. We may consequently assert that had syntactic or phonetic doublets evolved from *homo* or had the competition offered by the indefinite *si* been less effective, some form of *homo* in all likelihood would remain today in general use in Italian as an indeterminate pronoun.

MISCELLANEA

THE CONFUSION OF THE NEUTER *i̯a*-DECLENSION WITH THE FEMININE *īn*-DECLENSION IN OLD NORSE

Examples of substantives belonging to both declensions are *frelsi* (OHG *frīhalsī*: OFris. *frīhelse*) 'freedom', *hylli* (OS *huldi*: OHG *huldī*) 'favor', *frø'di* (Goth. *frodei*: OHG *fruotī*) 'wisdom'.

Since here only the corresponding *īn*-abstract and never the corresponding neuter *i̯a*-abstract is found in the other Germ. languages we must assume that likewise in ON the *īn*-declension represents the original declension and therefore the neuter *i̯a*-declension represents a secondary ON development.

The point of departure for the confusion of the two declensions was no doubt purely morphological, i.e., the congruence of the ending *-i* (*-e*) in all cases (except the genitive) in the singular paradigm to which the *īn*-abstracts were confined.

	Neut. *i̯a*-stem (long)	Fem. *īn*-stem
Nom.	*kvæd-i*	*hyll-i*
Gen.	*kvæd-is*	*hyll-i*
Dat.	*kvæd-i*	*hyll-i*
Acc.	*kvæd-i*	*hyll-i*

But there were undoubtedly semantic points of contact between substantives of the two declensions which contributed to the confusion in question.

These semantic points of contact were: (1) the abstract sense of certain *i̯a*-stems (cf. *edli* = OHG *edili* 'nature' with *hylli* 'favor'); and (2) the necessity for expressing a plural idea.

A plural form of the *īn*-substantives was felt necessary; (a) when the *īn*-abstract passed over into a concrete sense[1]; and (b) when the *īn*-abstract denoted[2] either a number of qualities, or a number of acts

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[1] For the displacement of the *īn*-declension by other declensions when the abstract has passed over into a concrete sense compare the gen. sing. ending *-ar* (*ō*-declension) in *gørsem-i*:*gørsem-ar* 'treasure', etc.; the plural *ō*-declension in *lyg-i*:*lyg-ar* 'a lie'; the plural consonantal declension in *rekend-i*:*rekend-r* 'fetter'.

[2] For plural abstracts in the other Germ. languages compare: Goth. (Ph. 4.8) *jabai hwō gōdeinō* 'ἀρετή'; OS (Hēliand 3370) *nu is mi thīnaro helpeno tharf*; OHG (Otfrid 1.21.15) *uuahs er in wizzīn*.

(iterative or customary action).   Examples are: (a) *frø'ði* 'wisdom' >
'wisdom recorded in writing' = 'records' (cf. *i frø'ðum þessum* 'in these
records', Skálda 3, Thorodd); (b) *nu's því lokit, numin eru frø'ði* 'Now
we have finished, the wise lore I have learned', Grp. 18.1.   Here *frø'ði*
nom. plur. 'wise lore' (spoken at various times), i.e., 'wise prophesy'
(given to Sigurth by Gripir in answer to Sigurth's *many questions*).

The plural forms *frø'ðum* and *frø'ði* in the two foregoing passages
represent a neuter *ia*-stem.   From the plural the neuter *ia*-stem could
have been transferred to the singular, thereby giving an additional
impulse to the already established confusion between the two declen-
sions in the singular, *frø'ði* fem. and neut. 'wisdom'.

Jóhannesson's contention[3] that the abstract suffixes *-læti*, *-mæli*,
-yrði, *-ræði*, *-lyndi*, *-leysi* represent original *īn*-stems which later passed
over into neuter *ia*-stems has no foundation.   If these substantives
represented original *īn*-stems we should have expected the *īn*-declension
to persist alongside the secondary *ia*-declension[4] exactly as in the case
of *frelsi*, *hylli*, *frø'ði*, etc.   As independent words *læti*, *mæli*, *ræði* and
lyndi all appear as neuter *ia*-stems, never as *īn*-stems.   The inflection
of the simplex has been retained in the compound and there is no reason
why we should not assume this to be true of the suffixes *-yrði*[5] and *-leysi*
which do not appear as independent words.   The use of these substan-
tives as suffixes has therefore no bearing upon the question.

Since the *īn*-abstracts were far more numerous than the neuter *ia*-
abstracts, it is safest to assume if an abstract appears only as a neuter
ia-stem that this represents the original declension.

There is no convincing evidence that an original *īn*-stem was ever
displaced by a neuter *ia*-stem.[6]   On the other hand original abstract
neuter *ia*-stems were sometimes displaced by *īn*-stems, e.g., in the case
of neuter *ia*-abstracts derived from strong verbs[7] of the 5th and 6th

[3] Alexander Jóhannesson, Die Suffixe im Isländischen §45.2.

[4] A parallel case is the confusion between the abstract *īn*- and *īn-i*-stems in
Gothic.   See Streitberg, Got. Elementarb.[5-6]   §157, Anm. 1.

[5] For *(laus-)yrði* Jóhannesson might refer to Goth. *(lausa-)waúrdei*.   But we
also have Goth. *(lausa-)waúrdi* neut. *ia*-stem (κενοφωνία) = ON *yrði* neut.
ia-stem.

[6] Except in the case of the type *hyggjandi* 'intelligence' > *hyggindi* neut. after
the pattern of the neuter *ia*-abstracts in original *-indi* (cf. *sann-indi* 'truth',
heil-indi 'health').   The formal congruence of the suffix syllable accounts for
this shift of declension.   It will be noted that the form *hyggjandi* never appears
as a neuter *ia*-stem.

[7] Cf. Wilhelm Cederschiöld, Studier över genusväxlingen i fornvästnord. och
fornsvenska (Göteborg, 1913); Chas. D. Buchanan, Substantivized Adjectives
in Old Norse, Lang. Dissertation 15.53 (1933).

ablaut series (e.g., *gaum-gæfi* fem. 'attention' (*gefa*), *van-færi* fem. 'lack of ability' (**-fara*); also *harð-fengi* fem. 'quarrelsomeness' of the reduplicating class **fāχan*, etc.). This displacement may be explained as due to the preponderance of the original *īn*-stems over the original abstract neuter *i̯a*-stems. The smaller category broke down in favor of the larger.

The fact that the *i̯a*-stems were never displaced by the *īn*-stems except in the case of verbal abstracts is sufficient evidence that here not only the formal but also the semantic congruity accounts for the shift of declension. Similarly we may infer that original *īn*-stems would never have passed over into *i̯a*-stems, resulting in a double declension (cf. *frelsi, hylli, frø'ði*), if the *i̯a*-stems had not likewise denoted abstracts.

The first step in the confusion between the two declensions was the transference of the *s*-ending of the genitive singular (the only case where the two declensions differed) from the *i̯a*-declension to the *īn*-declension (cf. *kristni*: gen. *kristni(s)* 'christianity'). From the genitive singular form in -*s* the *īn*-stem then suffered, after the pattern of the *i̯a*-abstracts, a shift of gender and therefore of declension (cf. *frelsi*: gen. *frelsi-s* neut.: hence *frelsi* neut.). This tendency was increased by the occurrence of the secondary *i̯a*-forms in the plural (cf. *frø'ði* neut. plur.: *frø'ði* sing. neut. and fem.).

<div align="right">A. M. Sturtevant</div>

OLD FRENCH DEMONSTRATIVES

The etymology of OF *icist, icil, ici, iço* from *ecce + iste, ille, hic, hoc* is widely accepted without discussion by Romance scholars.[1]

The phonetic difficulties presented by the initial *i* of the OF forms are generally ignored. Yet the presence of an *i* not derived from Lat. *ī* in the initial syllable of OF words is an extremely rare phenomenon.[2] Bourciez[3] claims that the change of *e > i*, which occurs in Italy, does not appear in Gaul; Guarnerio[4] claims the change *e > i* for parts of Italy, Moldavia, the Morvan and Walloon dialects and Asturias, but

[1] Cf. Diez, Et. Wb. d. rom. Sprachen, s. vv. *questo, quello, qui*; Bourciez, Eléments de linguistique romane 235, 251, 341; Grandgent, Introduction to Vulgar Latin 35–6; Nyrop, Gram. hist. de la langue fr. 2.391–402; Körting, Lat.-rom. Wb.; Bloch, Dict. ét. de la langue fr.; Brunot, Hist. de la langue fr., 1.192–3; Anglade, Gram. de l'ancien fr. 97–8, 147; Stappers, Dict. synoptique d'étymologie fr.; Clédat, Manuel de phon. et de morph. rom. 88.

[2] *lion < leonem*, caused by hiatus; *ciment < caementum*, caused by initial palatal; Anglade 28

[3] Op. cit., 153, 288.

[4] Fonologia romanza 338–9.

states[5] that in French this change occurs only before *n* or *l*. The same statement is made by Meyer-Lübke.[6] The latter author, who carefully refrains from offering the etymology *ecce* + *iste, ille, hic*, either in his Grammaire or in his Rom. et. Wb., refutes by implication the theory of a possible double retrogressive assimilation (*ecce iste* > *ecce ęstī* > *eccist* > *icist*) by a discussion[7] of the dissimilative tendencies of *ī* (*vīcīnus* > *vęcino, dīvīnare* > *dęvinare*, etc.) as borne out by OF development.

Diez, in his Et. Wb. d. rom. Sprachen, after presenting the possibility of *icelui* and *icelei* < *ipsi lui* and *ipsi illi ei*, appearing in the formularies of Marculfus and Mabillon, gives up the attempt on phonetic grounds (OF *c* cannot represent Lat. *s*, as proved by Picard *chelui, ichi*), and reverts to the *ecce* etymology, despite its obvious phonetic shortcomings, offering as proof a rare construction appearing in medieval writers (*parentes ecce habeo multos*).

Few other scholars attempt to solve the problem of the initial *i*. Anglade calls it prothetic: an unsatisfactory explanation, in view of the fact that the prothetic vowel in French is *e*, not *i*, and that nowhere else does prothesis appear in words of this type, beginning with palatalized *c*. Bourciez[8] and Clédat[9] both advance the hypothesis of the influence of the following palatalized *c* for the vowel change; but such influence is without parallel.

Schwan-Behrens is the only Romance scholar who faces the situation squarely. He states:[10] 'Une explication satisfaisante manque encore pour le premier *i* d'*icil* et d'*icist*', and again:[11] 'L'explication de l'*i* initial de la forme primitive présente également des difficultés, si l'on compare les transformations d'*ekke*, là où il apparaît comme mot indépendant' (*ekke* > *ez*).

In view of these apparently insoluble phonetic difficulties, a thorough investigation of the Vulgar and Medieval Latin texts of the 7th, 8th, 9th, and 10th centuries would seem to point to the possibility of a combination of *hīc* (either the adverb, or a standardized form of the demonstrative, extended to all cases, numbers, and genders) with *iste, ille*, and *hic*, the compound being phonetico-syntactically treated as a single

[5] Op. cit. 352.
[6] Grammaire des langues romanes 1.306.
[7] Grammaire 1.303.
[8] Phonétique française 129.
[9] Manuel de phonétique 104.
[10] Grammaire de l'ancien français 69.
[11] Op. cit. 195.

word, with palatalization of an intervocalic *c* placed between two front vowels (unlike *hoc ille* > *oïl*; cf. Grandgent 112 for the general retention of final gutturals in Vulgar Latin, and compare **plácere* > **plagere* > **plaiere* > *plaire* and *placére* > **platsere* > **plaitsieir* > *plaisir* for the double treatment of gutturals in Fr.). In the case of *hic hoc* > *iço*, the formation would be due to analogy. This analogy would seem at least as reasonable as that by which etymological dictionaries explain such formations as *itel, itant, idonc* (< *talem, tantum, deumquam* with initial *i* from analogy of *icist, icil, ici*). No analogical explanation is needed for *itel, itant, idonc,* if an invariable *hic* is accepted as the first member of the compound, and OF *issi,* usually explained as from *aeque sic,* can equally well be referred to *hic sic.* A real economy in the use of the doubtful analogical factor is thus effected by accepting *hic* in the place of *ecce.*

Combinations such as *ecce ille, ecce iste,* which appear in Plautus,[12] are not in evidence in the texts of the Vulgar Latin period; these on the contrary display, particularly from the 7th century on, a double use of *hic* which appears to point, on the one hand, to an extension of the standardized masc. sing. nom. (or the adverb), and on the other hand, to an ever more frequent combination with *iste* in the function of a true demonstrative (*ille* having lost demonstrative force by virtue of its frequent use as an article).

Gregory of Tours uses *hic* for *haec* (fem. sg.).[13] Vielliard, in her discussion of the Merovingian charters,[14] speaks of the extension of the form *hic* to the fem. singular and plural, particularly in those annotations on the reverse of the documents which more currently betray the spoken language. The same phenomenon is reported by Delisle.[15] In my own work,[16] I have reported frequent cases of a similar use (*hic sunt carctas, hic est emunitas,* etc.). It would therefore appear that there was a tendency, attested since the 6th century and becoming more widespread in the 7th and 8th, to extend the use of *hic* (adverb or demonstrative?) to cover the functions of other declensional forms, particularly *haec.*

[12] Grandgent 35–6.

[13] Bonnet, Le latin de Grégoire de Tours 387.

[14] La langue des diplômes royaux et chartes privées de l'époque mérovingienne, Paris, 1927, 145–6.

[15] Authentiques de reliques de l'époque mérovingienne, Mélanges de l'école française de Rome, 1884, 4.3.

[16] The Language of the Eighth-Century Texts in Northern France, N. Y., 1932, 173.

The combination of forms of *hic* with *iste* is strongly attested in the latter part of the 8th century, a formative and creative period for OF, and in the 9th and 10th centuries, when we may suppose that the etymological feeling of the original derivation of OF forms was not yet lost. I have shown[17] such expressions as *hanc donatione ista, contra hanc epistulam donacionis ista,* appearing in the latter part of the 8th century. Similar, but far more numerous forms are reported by Slijper,[18] who states in connection with his 8th century mss.: '*Hic* saepissime conjunctum invenies cum *iste; contra hanc securitate ista* passim, *hec vindicio ista*', etc. Beszard[19] reports for his 9th century mss.: 'Il est du reste fréquent de trouver *iste* accompagnant un autre démonstratif: *hanc cartolam ista, hanc cessione ista,* etc. Ces expressions pourraient être considérées comme relevant de la syntaxe; nous les mentionnons ici parce qu'elles rappellent de bien près les dém. doubles *ecce hic, ecce ille, ecce iste,* etc., qui sont les thèmes étymologiques de nombreux démonstratifs romans.' Morel[20] reports an abundance of *hic +iste* forms in the Cluny texts.

A more extensive investigation of the original documents of Tardif[21] and of the documents of Cluny[22] for the 8th, 9th, and 10th centuries brings to light the following facts:

In the royal deeds of Tardif, the scribes of the king's court, trained in the use of Latin since the reform of Pepin and Charlemagne, display better care in their handling of the demonstratives. The demonstrative function taken over by *ipse* in the 7th and 8th centuries (in the sense of 'the aforesaid') is to a large extent replaced in the 9th by *iste, hic,* and (more rarely) *ille* and *idem.* The combination of *hic* and *iste* is rare, though we have *presens donatio ista* (101) in 848, and the two words occurring in direct contact: *hec ista* (153) about the year 1000. On the other hand, we have an interesting extension of *hic* (142) in 916: *hic sancto loco dare;* and, toward the end of the 10th century, when we may suppose that the etymological sense of the derivation of *icist* was beginning to be lost, such combinations as *illi ipsi fratres, his ipsis hic, eis ipsis, illis ipsis* (153).

On the other hand, the private documents of Cluny, written by less

[17] Op. cit. 198, 201.

[18] De Formularum Andecavensium Latinitate Disputatio, Amsterdam, 1906, 115.

[19] La langue des formules de Sens, Paris, 1910, 32.

[20] Ecole des chartes, positions des thèses de 1914, 77–82.

[21] Monuments historiques, Paris, 1866.

[22] Bernard, Recueil des chartes de l'abbaye de Cluny, Paris, 1876, I–II.

cultured scribes, display an impressive series of combinations throughout the 9th and 10th centuries: *hanc vindicione ista*, 845 (8); *anc vindicione ista*, 870 (16); *ec condonacio ista*, 903 (90); and countless others. Erroneous uses of forms of *hic*, such as *oc vindicione ista*, 906 (210); *ec dotalio isto*, 928 (177); *ec scamius istus*, 920 (210); *anc dotalicio isto*, 928 (335); *ec ipso campo*, 915 (187), attest the loss of declensional sense for *hic* which we might expect if *hic* had been previously standardized in speech to the exclusion of all other declensional forms. In an especially poorly written document of the year 881 (29), we find the two words in direct contact: *hanc ista vindictione, ehc ista vindictio.*

It is worthy of note that in the vast majority of cases where *hic* and *iste* are combined, the two forms are separated by the interposition of the noun. This is possibly a conventionalized concession made by the scribes to classical usage, a compromise between the directly combined *hic iste* of their speech and the knowledge existing in their minds that classical Latin regularly makes use of a single demonstrative. From the outset of the *hic* + *iste* vogue, in the second half of the 8th century, until the end of the 10th, it is only exceptionally that the two forms appear in direct contact. But this, occurring as it does at a period when the scribes had been definitely made conscious of Classical Latin by the Carolingian Renaissance, is not a factor of sufficient importance to cause us to reject the theory, attested by the extension of *hic* since the 6th century and by the combining tendencies of *hic* and *iste* since the 8th, that it may have been a standardized *hic*, in close phonetico-syntactic combination with *iste, ille, hic*, and *hoc*, that gave rise to OF *icist, icil, ici, iço*, rather than an *ecce* which is at variance with the phonetic development of French.

This hypothesis, resting as it does upon evidence supplied by the texts of the Vulgar and Medieval Latin periods, points once again to the importance of a thorough study of those texts when attempting to solve difficult or controversial points in Romance development.

MARIO A. PEI

ETYMOLOGY OF FRENCH POTIRON

Modern Provençal *boutareu, poutarel, poutaro* 'mushroom', and French *potiron* 'edible mushroom', 'pumpkin' (also appearing in the phrase *courge potiron* 'pumpkin') are derived by Schuchardt[1] from Arabic *fuṭr* 'toadstool'; but he recognizes that Arabic *fā* should give

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[1] Z.f. rom. Phil. 28.156; recorded without dissent by Meyer-Lübke, REW³.

French and Provençal *f*, not *p*; and his conjecture that the Arabic word, which has Semitic cognates like Aramaic *peṭūrotā*, may have been brought to France by Jewish or Arabic physicians, does not suffice to render the etymology phonetically or historically plausible, notwithstanding Gamillscheg's proposal[2] of the indefensible Syriac cognate *pāṭūrtā*, which he sets down as the etymon of French *potiron*.

I posit the Vulgar Latin type **pottarellu* > **pottu* 'pot' plus the double suffix *-arellu*. Adams[3] gives three adjectival examples of the suffix *-arel*. The occurrence of the suffix in Gallic territory is also indicated by the French examples of the suffix *-rellu* adduced by Meyer-Lübke.[4]

The original sense was doubtless adjectival: 'pertaining to the pot', whence 'vegetable for pottage', 'mushroom', 'pumpkin'.

The *u* (*ou*) *o* in all the Provençal forms is regular, as in *pourta* < *portare*, *dourmi* < *dormire*. The initial consonant of the variant *boutareu* may easily be attributed to the influence of *boutarel* (Old Provençal *botarel*) 'little cask'. The variant *poutaro* is due to the dropping of *-ellu*, the stem left being *pottar-* instead of the ancient *pott-*.

French *potiron* evidently contains the same stem with an augmentative suffix instead of the diminutive suffix used in southern France. In view of the French dialectic forms in *-uron*, the etymological type was perhaps rather **pottūrōne* than **pottărōne*. The irregular pretonic vowel of *potiron* seems due to some associative interference, such as that of *petit rond* 'little circle'.

<div style="text-align: right">C. C. Rice</div>

ON LAVES' REVIEW OF DEMPWOLFF

Having read both Volume 1 of Dempwolff's Vergleichende Lautlehre des austronesischen Wortschatzes and the review of this volume in LANGUAGE 11.264ff., I believe it is only fair to say that Dempwolff's volume seems to me to be a serious and very helpful contribution to Indonesian comparative grammar. The data available today probably would not suffice for a complete history of sounds; so far as Dempwolff has gone, he has worked with his usual accuracy and skill. Your reviewer mentions no errors of fact or comparison (in particular, there seems to be no conflict between the statements about nasalization given by Dempwolff on page 30, and those cited by your reviewer, 265). Instead, the reviewer merely voices theoretical considerations of doubt-

[2] Et. Wtb. d. fr. Spr., 1928.
[3] Word-formation in Provençal 402.
[4] Rom. Gram. 2.545.

ful relevance. The correspondences cited by Dempwolff are much more than 'fairly plausible', and 'the nature of the genetic relationships. of these languages to one another' does not 'remain undetermined', but has been determined by a succession of scholars, among whom Dempwolff holds an honorable place.

L. BLOOMFIELD

ON VARIOUS PHONEMES

Morris Swadesh, in LANGUAGE 11.244–50, has subjected my monograph, On Defining the Phoneme, to a thorough and penetrating review. He has quite properly omitted useless repetitions in points upon which we patently agree, and has devoted himself to an exposition of points of disagreement. I hope it will not be interpreted as incorrigible obstinacy if I attempt to stand my ground on many of the points at issue, particularly those in which I believe Swadesh's criticisms might lead to misinterpretations of my true position. The presentation of rebuttals is complicated by the technique which Swadesh occasionally uses: an inadequate presentation of my position, confronted by an authoritarian repetition of his own earlier pronouncements with little or no additional arguments to support them, e.g., on a positive vs. fictional phoneme, on the p's of *speech* and *spill*, and on 'pattern congruity'.

Swadesh notes (245) that my definition 'establishes a concept which is not the phoneme and which, in my estimation, is not likely in its present form to serve any important scientific function'. On the first point, I believe I enjoy the distinction of being the only writer on the subject who is not convinced that his own particular phoneme is the phoneme; cf. Monogr. 52. On the second point let us take a specific case: Swadesh (246) sets up a language in which we find 'in open syllables: [i, e, ɛ]; in closed syllables: [ɪ, ɛ, æ]. If this summarizes all the pertinent data, a correct theory should require the recognition of just three phonemes:

	Norm in open syll.	Norm in closed syll.
(a)	[i]	[ɪ]
(b)	[e]	[ɛ]
(c)	[ɛ]	[æ]'

Now it is clear that according to the principles expounded in my monograph (46ff.), this and only this required arrangement must be arrived at, since the differential relations of the two classes of vowels are similar

and in a one-to-one relation.   Swadesh's own methods, however, cannot
possibly lead to this arrangement.   He has written (LANGUAGE 10.123):
'If the distribution of one type of sound is complementary to that of
more than one other, it is to be identified with one rather than the other
if there is a more definite phonetic similarity in that direction.'   The
distribution of [ɛ] in closed syllables is obviously complementary to that
of [i] and [e] and [ɛ] in open syllables.   Swadesh must now search for a
'definite phonetic similarity'.   And he will have no choice: he must
combine the closed-syllable [ɛ] of phoneme (b) with the open-syllable
[ɛ] of phoneme (c), on his principle of phonetic similarity.   If a phoneme
definition is to be judged by its success in this example of Swadesh's,
I hint, as unobjectionably as possible, that mine 'serves that scientific
function', and his does not.

In his general discussion of the phoneme (245), I miss any attempt
on Swadesh's part to defend his purely verbal, dictionary type of defini-
tion as against a procedural or operational definition.   It seems to me
beyond question that a useful definition will not merely mark off the
phoneme in general from a taxeme or a morpheme, but will identify a
given phoneme as such when it has been discovered or determined.   A
phoneme is not merely different from a morpheme, it is different from
another phoneme.   Swadesh's definition ('an elemental type of speech
sound in a given language') does not specify the nature of that differ-
ence; mine, I believe, does.

Swadesh writes at some length (247f.) on the disposal of the stop in
spill as a variant norm of his *p*-phoneme.   I may say that my discussion
of this point in the monograph (30f.) was entirely within the frame of
the data supplied by Swadesh in his earlier writing.   Swadesh now wishes
to adduce two further considerations to justify his combining the stops
of *pill* and *spill* in a single phoneme: (1) The stop of *spill* isn't after all
quite so weakly articulated as a [b], though in some cases it may be.
(2) S. S. Newman has discovered that the contrast of *respect/asbestos*
indicates that a differentiation of [p]/[b] after [s] does exist in American
English.   On the first point, I see no importance in this new attitude
toward the strength of articulation.   Swadesh still admits that, on his
principle of phonetic similarity, the stop of *spill* resembles a [b] with
respect to strength of articulation.   With respect to aspiration, the
same is true.   His phonetic similarity between the stops of *pill* and
spill is as before in the single category of voice.   The contrast of *respect/
asbestos* is not clear to me.   Are we to understand Swadesh as asserting
that in these words the [p] and the [b] occur in similar phonetic contexts?

The radical differences in accentual pattern and syllable division make it inconceivable that the two environments should be regarded as identical. Further, the pronunciation of *asbestos* (as a popular word) with a definitely voiced sibilant, as soon as the pattern approaches that of *respect*, is worth pondering.

In this connection, Swadesh observes (247): 'I may add here that it is my experience that states of perfect balance, where a sound falls phonetically exactly between two others . . . , are rare.' The turn of phrase here illustrates, I believe, a basic unstated assumption of Swadesh's: that phonetic similarity is one-dimensional and absolute, that precise degrees of phonetic similarity or difference can be objectively determined and unambiguously indicated by points on a straight line. Only in terms of some such unconscious assumption could one speak of a sound as falling 'phonetically exactly between two others'. Phonetic similarity is a complex of many factors, not immediately comparable with one another; much depends upon the selection of one particular dimension in which similarity is to be found; and that selection must in some cases be arbitrary. This basic assumption weakens, I believe, Swadesh's criterion of phonetic similarity as a sovereign solution of problems in phoneme distribution, quite apart from the disastrous results of its application to his hypothetical language above. Further, we are not here concerned with hypothetical cases of 'perfect balance', but with certain actual cases in which phonetic similarity is not decisively in the one direction or the other. If one cannot measure precisely, a lack of decisive similarity prevents a solution on Swadesh's principle just as effectively as a 'perfect balance'.

In Swadesh's defense of his 'criterion of pattern congruity' (248), I miss any attempt to justify the assumption that there is an a priori phonemic system or pattern. Until he establishes such a system as existent, and existent apart from the phonemic relations, I must continue to regard his 'criterion of pattern congruity' a dangerously circular one to apply in the determination of particular phonemic relations.

Swadesh remarks (248) of certain functional definitions: 'Twaddell does not offer any specific criticism of them, unless he feels that his remarks on psychological-reality definitions cover them, too.' It may be proper to note that the final paragraph of Monogr. 15 indicates that I do so feel.

Of Swadesh's defense of Sapir, there is little I can say, except to state that I cannot interpret Sapir's recent writings on this and related subjects other than as indicating a conviction that psychological realities

are pertinent to the study of linguistics, that phonemes can be demonstrated if not determined on psychological grounds, that phonemes coexist in mental series, as a 'feeling in the bones'.   If I misinterpret him, it is without malice.

Swadesh's objections (249) to the failure explicitly to justify the term 'minimum' are, I believe, well taken.   I hoped it would be clear to those interested in the phoneme who might read the discussion that what was there presented as a straight-line development would inevitably, in practical work, be accomplished only at the cost of considerable trial and error.   Swadesh properly notes that it would be possible, taking such a series as *apple, staple, grapple, nipple, supple,* etc., to arrive at a class of microphonemes [æ, ste, græ, nɪ, sʌ], etc.   Of course, such a series might be useful, e.g. from a morphological point of view.   But I should have made it clear that I was concerned with the totality of American English forms, not merely those ending in [pəl].   The possibility of regarding [pɪ/bɛ] as a minimum difference (between *pit* and *bet*) is of course removed as soon as one discovers the forms *pet* and *bit*; cf. Monogr. 43.

The ingenious table which Swadesh presents (250) is quite another matter.   Here Swadesh represents me as advocating starting in the middle of my procedure and working both ways at once.   A careful rereading of the monograph reveals to me no justification for ascribing to me any principle which could produce such an assemblage of the irrelevant and misleading.   If one restricted one's attention to the series *kill: till: pill: hill,* one could arrive at alternative microphonemic classes [kh], [th], [ph], [h] or [k], [t], [p], zero.   But I find it hard to imagine any linguist working with quite so narrow a universe of discourse; and the addition of the form *ill* at once rules out the second alternative.   One would, I assumed, record the forms as accurately as possible; then, working with these and a multitude of other forms, one would discover no significant differentiations [l/L], [r/R], [k̂/k], [kh/k], etc.   It may be pointed out that the only real difficulty presented by Swadesh's table arises in the group *kill: skill;* and there one must query the a priori assumptions—on the basis, probably, of 'phonetic similarity' —(1) that the aspiration of [k] in *kill* is identical with the glottal fricative in *hill,* and (2) that the '[k]' of *skill* is identical with the [k] of *kill,* except for the aspiration.

It is natural that Swadesh should find (250) that 'the weakness in Twaddell's macro-phoneme is its failure to recognize phonetic similarity as a criterion'.   It is also natural, and perhaps pardonable, that I should

find the strength in my suggested procedure precisely in its substitution of a principle of differential correspondences for the earlier reliance upon an undefined and indeterminable (and sometimes misleading) entity, 'phonetic similarity'. All of us are agreed, for example, that from *pin/tin* we may deduce a phonemic differentiation [p-]/[t-]; from *nip/nit* a phonemic differentiation [-p]/[-t]. How can [p-] and [-p], or [t-] and [-t], be brought into relation with each other? The earlier procedure has been to say that they must be identical, that the intuition of phonetic similarity is overwhelming. That intuition may or may not be 'correct'; the fact remains that an intuition has no business in a science, if we can get along without it. Until a positive unit of phonetic similarity emerges from the laboratory, I can see no better criterion for phoneme grouping than that of differential correspondence.

Swadesh summarizes (250): 'Twaddell's theory would declare as different: the vowels of *sing* and *sin*, of *length* and *Len*, of *long* and *lawn*, of *sung* and *sun*, or *sang* and *man*, the *s*'s of *still* and *sill*, the *t*'s of *gust* and *gut*, the *r*'s of *tray* and *buttress* (the latter contrasts with *l* as in *butler*, the former cannot contrast with *l*), etc., etc., etc.' I should be interested to know by what processes Swadesh arrived at some of the items in this list; but this is not the place for that discussion. It is not too much to say, however, that it is no 'theory' of mine, but commonplace observation, which recognizes as different the vowels of *sing* and *sin*. The question is whether the phonemes involved should be identical, and whether a principle which recognizes the objective difference is necessarily weaker than one which ignores it. Swadesh finds it convenient so to define the phoneme that the difference is ignored; he can thus write a language with relatively few symbols. But for the study of the language, he will have to take into account just those differences which he has earlier chosen to ignore; where I have separate phonemes for *sing* and *sin*, he will later have to recognize two variant norms of his single phoneme, and where I have different phonemes for the stops of *pill* and *spill*, he will have one phoneme, but two variant norms. I cannot see that 'simplicity of total formulation' (Swadesh 250) is overwhelmingly on one side or the other. I maintain that for some purposes it is convenient to regard the phonemes involved as different: *sin* is different from *seen*, a type of relation in which *sing* cannot participate, in the American English language. The example of *tray*, *buttress*, and *butler* involves again the factor of syllable division; Sapir's article (263 f.) quoted by Swadesh, furnishes the proper clue here. On this basic issue Swadesh and I go separate ways; the reasons for my choice are outlined

in the sixth section of my monograph; until Swadesh takes issue with
that argument and refutes it, I can offer no further defense.

What Swadesh wants, I believe, is a set of some 30 to 60 units, 'ele-
mental types of speech sound', which can be combined in various ways
to build up 'words', 'forms'. Those units, those ultimate building-
blocks, he wants to call phonemes. For him, they would be the elements
of the language, since they are the elements of its forms; the forms
would be secondary, derived complexes. The phonemes would be
atoms, and the forms molecules. This is a respected approach, and
adequately sanctioned, by tradition and wide acceptance. It leads to
difficulties, as Chao has pointed out (Bull. Inst. Hist. & Philol., Aca-
demia Sinica 4.4.363–97); but it does good work, e.g. in establishing
sanctions for transcription, providing a theory of phonetic change, etc.

But there is another point of view, explicitly presented by Jespersen
almost a half century ago, and an organic development, I believe, of the
essential core of truth in the Junggrammatiker doctrine. According to
this view the smallest real unit in language is the form, preferably the
free form, in Bloomfield's terminology. The free form or 'word' stands,
potentially autonomous and indivisible, as the object of study. When
we dissect it, we find segments that are not linguistic—i.e. communica-
tory or expressive—units. Now a form is related, from the point of
view of the phonetic events to which it corresponds, to many other
forms of the language; the study of these relations (which must be differ-
ential, except for homonyms) is a study in which the entity I have de-
fined may, I hope, be useful. For that kind of study we cannot operate
with a minimum set of elemental constituent phonemes; we must have a
specifically relational, abstractional unit, which is explicitly defined as
the term of a relation. Only similarities in the relations can be valid;
not phonetic similarity of the units themselves. If we are studying,
say, the relation of older sister and younger brother, we cannot bring into
that study only child John Smith, age 18, height 5′9″, weight 150, eyes
brown, etc., merely because William Jones (who has an older sister) is
aged 18, height 5′8″, weight 145, eyes brown, etc. The physiological
similarity may be valid for statistics on military service, but has no
significance in a study of sibling-relationships. Phonetic similarity is a
valid criterion for some purposes; but in many fields of linguistics, the
differential relation is crucially significant. *Sing* is an only child; *sin*
has an older sister *seen*.

Swadesh concludes his review of my study: 'In emphasizing the im-
portance of systematic gaps of distribution, it fails to recognize the

systematic nature of phonetic resemblances.' To this there can be no reply beyond a request to those interested to read my study, with the further note that if the criticism were just, I should be expelled from the LSA.

Penultimately, I record my regret at giving the impression of translating (erroneously or otherwise) anything in Sapir's article as 'experiments'. That term I used as a not unduly dignified synonym for 'incidents', 'cases'—what Swadesh calls 'anecdotes'.

Finally, I recall that in the preparation of my monograph I was at considerable pains to say neither more nor less than I meant, and I hoped that those readers who had a real interest in the problem would by careful reading be able to discern my position. In some important respects I seem to have failed; I record my gratitude to Swadesh for indicating so clearly the points upon which I left my position unclear and open to misinterpretation, and for making it possible for me perhaps to correct some mistaken impressions.

Since early in October, Swadesh and I have been in correspondence on the issues raised in his review and my attempt at rebuttal above. Swadesh wishes to have two points here mentioned, in connection with the discussion: (1) He proposes not *absolute*, but *relative* phonetic similarity as a phonemic criterion, and now offers a more unambiguous statement: Micro-phonemes that, on the basis of their phonetic nature, occupy a like place in comparable series are equivalent. (2) He accepts 'totality of forms' (introduced above) as a satisfactory basis for determining 'minimum phonological difference among forms'.

W. F. TWADDELL

SECOND INTERNATIONAL CONGRESS OF PHONETIC SCIENCES, JULY 22–26, 1935

University College, owing to the untiring efforts of Daniel Jones, President of the Congress, an important seat of phonetic studies for many years, was a generous host to this gathering of scholars from all parts of the world. The active participation in the proceedings may be inferred from the number of papers presented by scholars from the various countries: England and Scotland 19; Germany 11; the United States 9; Scandinavia 5; Austria, Czechoslovakia and France, 4 each; Holland, India, Poland, 3 each; Japan, Spain, 2 each; Chile, China, Ireland, Russia, Switzerland, 1 each.

To the reporter the most interesting papers were those dealing with 'phonemics' or 'phonology', as the Prague group call it (rather unfortunately, I think), which were presented in a joint session with the

Internationale Arbeitsgemeinschaft für Phonologie; and those concerned with experimental phonetics offered in a joint session with the
International Society of Experimental Phonetics. I shall comment
briefly on the most noteworthy contributions in these two fields.

V. Bröndal (Copenhagen) pointed out that phonemes are ideal constituent parts of a pattern and that they are regular (constant), indivisible, and fixed in number in any given language; whereas 'sounds'
are real events, individual (variable), divisible, and innumerable in any
given language.

N. Trubetzkoy (Vienna), the learned and clear-headed leader of the
'phonologists', presented a stimulating discussion of phonemic and
unphonemic boundary marks (phonologische Abgrenzungsmittel).
Some boundary marks between words, he pointed out, are phonemic,
others not. The phonemes [j] and [w] invariably mark the beginning
of a word (simple or in composition) in the native elements of English;
so do [j] and [v] in German. They are phonemic (phonematisch)
boundary marks. The glottal stop in German also marks the beginning
of a word, but it is unphonemic (aphonematisch) since it can be omitted
without hindering intelligibility. In German a vowel preceded by a
glottal stop is merely the normal variant of the vowel in stressed initial
position. It has the same status as aspirated [p, t, k] in German (or
English), which also serve as unphonemic boundary marks (i.e. they
signal the beginning of a word). Stress in native English and in German words is also an unphonemic boundary mark, according to Trubetzkoy. Such boundary marks, both phonemic and unphonemic, are
found in all languages. They are not indispensible but they constitute
a welcome expedient (Notbehelf).[1]

K. Bühler (Vienna) pointed out that psychologically the phoneme
is a 'constant' in the same sense as size is a constant. When we see a
house nearby we take it to be of a certain size; when we see the same
house from a (known) distance we judge it to be of the same size although the image on the retina of the eye is much smaller. Similarly a
[t] in different contexts or uttered under different accentual conditions
or pronounced by a child and an adult, respectively, in an English
speaking community is taken to be 'one and the same sound' although
it varies in its physiological, physical, and auditory character.

R. H. Stetson (Oberlin) defined the phoneme as a 'movement pattern'.

[1] N. Trubetzkoy has just published an excellent 'Anleitung zu phonologischen
Beschreibungen'. Association Internationale pour les Études Phonologiques;
édition du Cercle Linguistique de Prague. Brno, 1935.

It is like any movement, no more, no less. There are two separate chains of movements in speaking: articulations (phonemes) and syllables. These two chains act upon each other. The articulations influence 'the run of syllables', and vice versa, but they are separate. The auditory factor is completely disregarded in this definition.

Alf Sommerfelt (Oslo) raised the question whether syllabification can be phonemic. As an example he cited three distinct words from a Scandinavian dialect: [bun-nə, bun·-nə, bun-n̩-nə].

The papers dealing with phonemics exhibited considerable diversity of opinion and left one with the impression that various phases of phonemic theory are still very much in need of clarification.

In the field of experimental phonetics the outstanding contribution was the X-ray sound film by P. Menzerath (Bonn) of the utterance of words and phrases. Movements of the larynx, the soft palate, the tongue, the jaw and the lips are all clearly visible in this remarkable film. His approach to the definition of speech sounds may be inferred from his dictum: 'Es gibt keine Stellungen, es gibt nur Bewegungen'.

T. S. Flatau (Berlin) discussed the gradual refinement of his laryngostroboscope. G. O. Russell (Ohio State) presented his well-known views of the physiological vowel scheme.

A timely warning concerning recent trends in instrumental phonetics was sounded by E. W. Selmar (Oslo), who fears that this discipline, with one foot in natural and one in historical science (linguistics), is losing its footing in linguistics. The need of intimate collaboration between instrumental phonetics, linguistics, and 'sprachphilosophie' had been ably voiced in an earlier session by W. Horn (Berlin) in a paper entitled 'Experimentalphonetik und Sprachgeschichte'.

Students of American English were well represented on the program of the Congress.

H. Kurath (Brown) gave a brief account of the Linguistic Atlas of New England (illustrated with lantern slides). He pointed out the need of a linguistic atlas of the British Isles for studies in British English as well as for investigations in the linguistic geography of American English and the historical relation of American English to British English.

M. L. Hanley (Wisconsin) discussed phonographic recording with special reference to his experience in field recording.

G. S. Lowman (Linguistic Atlas and University of Virginia) described the regional distribution of the several types of the [au] phoneme in Virginia, in its relation to the history of its settlement.

Various features of American English were discussed by J. S. Kenyon (Hiram): C. M. Wise (Louisiana), and Mrs. Jane D. Zimmerman (Barnard).

The Congress charged a committee consisting of Professor W. Horn (Berlin, chairman, Professor M. L. Hanley (Wisconsin), Professor H. Orton (Arnstrong College), Professor H. Kurath (Brown), and Professor J. Orr (Edinburg), to psepare a memorandum concerning a Linguistic Atlas of the British Isles, which is to be presented for consideration to the Royal, the Welsh, and the Irish Academies.

After the conclusion of the Congress, the International Phonetic Association held a meeting commemorating its fiftieth anniversary. Professor Danial Jones gave a brief account of the history of the arganization and its notable accomplishments. Of the founders of the Association only O. Jespersen was present.

The proceedings of the Congress will be published shortly.

Hans Kurath

In the Indian section, over which Prof. S. K. Chatterji of the University of Calcutta presided, the following papers were delivered: Mr. K. Battacharjee, Delhi, sent a paper which was read by Prof. Daniel Jones, on 'Articulations of Birds', especially parrots, cockatoos, and mynas. Prof. Chatterji read a paper on 'Phonetics in the Study of Classical and Sacred Languages in the East'. He called attention to the element of phonetics in the elementary learning process of Hindus following upon their scientifically arranged alphabets, to the various systems of gestures accompanying the tonal phenomena of the Vedas as they are chanted and sung, to the treatises on the phonetics of Arabic and Persian as studied in India, and the various shifts to which Indian Moslems are put in their efforts after correct pronunciation of their sacred texts, and to the Chinese and Japanese transcriptions of Sanskrit. Mr. J. R. Firth, University College, London, spoke on 'Phonological Features of some Indian Languages', applying phonemic principles to various phonetic features, especially contrasting the consonant systems of the Sanskritic vernaculars and Tamil. Dr. T. Grahame Bailey, School of Oriental Studies, London, read a paper on 'The Fourfold Consonant System of Kashmiri', in which he described the velarized, palatalized, and labialized consonants which are due to absorption of the qualities of final vowels now lost. Mr. A. C. Sen of Northampton, Mass., read a paper giving the results of 'An Experimental Study of Bengali Occlusives'.

M. B. Emeneau

BOOK REVIEWS

Indogermanisches Jahrbuch 19. Pp. 375. Edited by Albert Debrunner and Walter Porzig. Berlin and Leipzig: Walter de Gruyter & Co., 1935.

The volume contains the bibliography for 1933; and is, like its predecessors, an indispensible tool for all workers in its field. Its construction is so well known that no description is required beyond a listing, honoris causa, of the self-sacrificing men who have contributed to it: General Linguistics, W. Brandenstein, P. Meriggi, W. Porzig, H. Spehr, G. Ipsen; Indo-European Linguistics, W. Brandenstein; Indo-European Antiquities, H. Krahe; Tocharian, W. Printz; Aryan, W. Printz; Armenian, V. Inglisian; Albanian, N. Jokl; Greek, P. Chantraine; Italic, G. Devoto; Keltic, J. Pokorny; Germanic, G. Streitberg; Balto-Slavic E. Fraenkel, O. Hujer; Hittite and other ancient languages of Asia Minor, J. Friedrich.

The volume contains also biographies of Hermann Jacobsohn and Edmund Kleinhans, as well as news items of personal and scientific interest.

Two matters of a practical nature may be mentioned in this connection. The attention of contributors to Language is directed to the list of abbreviations (372–5) which may (with rare exceptions: AJP, Lang.) be followed or imitated. Especially noteworthy are such as HeidelbSb, MünchSb, WienSb which are every bit as effective as the two lines of print that are frequently desired for references to similar publications.

Finally I would mention the aid we can and should give the publication either by joining the Indo-Germanische Gesellschaft, by purchasing the Jahrbuch, or by securing an order for it from a library. The price is 18 Reichsmark (20 for a bound copy), and is small in comparison to the value of the work. The printing of such a volume is expensive, the funds of the Gesellschaft are limited; it has for some time been necessary to restrict the amount of material published, and the time may come when our support will be essential for the continuance of the enterprise. We are, of course, laboring under financial difficulties, but so too are the scholars of Europe. We profit by their labors, and may well share their burdens.

<div align="right">George Melville Bolling</div>

Atti del III Congresso Internazionale dei Linguisti (Roma, 19–26
Settembre 1933-XI).   Pp. xvi + 449.   Edited by Bruno Migliorini
and Vittore Pisani.   Firenze: Felice Le Monnier, 1935.

We have here the proceedings of the Third International Congress of
Linguists, in which representatives of thirty-two countries participated;
we regret to note that only one American, our fellow-member John
Kepke, crossed the ocean to attend it.   But when European meetings
are set late in September, the date is prohibitive for the attendance of
American scholars who have University and College positions.

The volume contains a great variety of communications presented in
four languages: apart from addresses of welcome and the like, and the
discussions following the papers, there are 30 communications in French,
making 120 pages; 28 in Italian, 116 pages; 27 in German, 109 pages;
and 3 in English, 17 pages.   In character they vary from questions of
the widest bearing, to very specialized points, and from those which
are admirably adapted to public reading, to those which are chiefly
lists of words equated with one another.

Any review would have to be of the separate papers, for which there is
no space here; and I therefore merely call attention to this volume and
its contents, that it may not be overlooked by those of us who for various
reasons could not attend the Congress.

<div align="right">Roland G. Kent</div>

**A Union List of Printed Indic Texts and Translations in American
Libraries.**   Pp. xv + 540.   By M. B. Emeneau.   New Haven:
American Oriental Society, 1935.   (American Oriental Series 7.)

The list was compiled and published with the financial backing of the
American Council of Learned Societies.   It includes all works in San-
skrit, Pali, Prakrit, and Apabhraṁśa, as well as most of those in the
earlier stages of the vernacular, that were printed before the end of
1933, and are in the possession of the larger libraries of the United
States (except the library of the University of California) or in the
library of McGill University.

The value of such a publication is at once evident, but I may illustrate
it from my own experience.   My first intention was to print in Studies
in Honor of Maurice Bloomfield (1920) the text of the *Laghucāṇakyam*
from the Galanos MS of the Vatican which I had copied in 1907.   It
was uncatalogued and quite securely buried, and I hoped that no prying
Sankritist had seen it.   In using Boehtlink's Indische Sprüche, how-

ever, I became convinced that such was not the case. Still a bibliographic search brought no edition to light, and two of our most eminent Indologists assured me that there was none. Only the diligence of an assistant in our library brought in the edition by Teza (now listed as no. 1045), and thus saved me from an exhibition of my ignorance. Had this work then been available, much trouble would have been saved, and I could have used also nos. 877, 1037, 1038, 1039, 1048, 3630 in preparing the article I substituted.

The editor very modestly assumes that there must be errors and omissions. I am in no position geographically to check this; but feel confident that they will prove at least to be of minor importance. As such I may mention the following. Either in New Haven or in Washington is an edition of the *Vṛddhacāṇakya* published at Bombay in 1847 of which I find no mention. It is about a decade older than the edition used by Weber (no. 1043), Boehtlink, and Kressler, and somewhat superior (cf. Studies in Honor of Maurice Bloomfield 52). Nor is it easy to believe that of the Cāṇakya material listed the AOS library contains nothing, and the Yale library only nos. 879, 1047.

<div align="right">George Melville Bolling</div>

Griechische Grammatik. Erste Lieferung: Allgemeiner Teil und Lautlehre. Pp. xxvii + 414. By Eduard Schwyzer. (Handbuch der Altertumswissenschaft, begründet von Iwan von Müller, herausgegeben von Walter Otto; zweite Abt., erster Teil, erste Lief.) München: C. H. Beck'sche Verlagsbuchhandlung, 1934.

Karl Brugmann's Griechische Grammatik appeared in four editions in Iwan von Müller's Handbuch der klassischen Altertumswissenschaft; the fourth edition was a revision by Albert Thumb, and appeared in 1913. The present work is in no way a revision of the 1913 edition but is an entirely new work: where Brugmann-Thumb devoted 26 pages to the introduction, Schwyzer gives it 163 pages; where Brugmann-Thumb had 161 pages of phonology, Schwyzer has 246.

But the differences are not merely in the amount of space devoted to the topics. New topics are introduced in the introduction, especially, of the new work, and there is an entire rearrangement of the treatment of the topics common to both. A summary, then, of the topics as given by Schwyzer is not out of place: history of the study of Greek as a language, from ancient times to the present day (4–11); linguistic theories

and methods, including an excellent[1] exposition in favor of phonetic
regularity (11–28); the tasks of lexicography and etymology in connec-
tion with Greek (29–45); the external history of the Greek language
(45–137), including genetic relationships and relation to neighbors and
to substrates, Proto-Greek and the Greek dialects, the ancient literary
dialects, the koine, the atticizing reaction, the development through
Byzantine times down to the spoken speech of today; the history of
writing in Greek lands (137–50); the evidence of other languages as
given by borrowings and transcriptions (150–165; with an immense
amount of detail).

The part on phonology includes the general phonetic presuppositions
(169–73); a general account of the normal pronunciation of the language
(174–7); the history of the Greek sounds from the general Greek status
(including dialectal variations) down to the present day (178–234),
followed by a section on syllabification, hiatus and contraction, assimila-
tion and dissimilation, metathesis, and similar phenomena, and changes
in consonant-groups and the like (234–89); the pre-history of the Greek
sounds, in the order stops, spirants, liquids, nasals, semivowels, gemi-
nates, other groups, vowels, diphthongs, ablaut (290–371); accent and
quantity (371–95); sandhi and phrase-end (395–414).

It will be noted that Schwyzer departs from the conventional order
in the consideration of the sounds; in this I sympathize with him, for
the order employed by Brugmann in the Grundriss is not necessarily
desirable for all languages, and is in fact highly disadvantageous for
some. But the division of the treatment of the sounds into two parts,
that from the general Greek status onward (178–234) and that before
that time (290–371), brings certain disadvantages in that it splits the
history just at the most interesting point, and there are many phenomena
which do not, strictly speaking, belong to one section rather than to the
other. We note also, that in the pre-history Schwyzer works backward
from Greek to the sources, a reversal of the true direction of historic
development which I have always found to obscure the aims in view.
Yet these considerations do not weigh very heavily with me in the
present instance, for I have found the volume to be so rich in content,

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[1] [I should abate the praise considerably: the section will not stand com-
parison with Bloomfield, Language 346–68. The omission of this work is the
most glaring defect I have noted in Schwyzer's bibliography. Schwyzer has
read Bloomfield's review of Hermann (LANG. 8. 220–33) without profiting fully
by it; nor has he learned from Hirt, Idg. Gram. 1. 130 how to deal with Horn.
GMB.]

so detailed in its data, so fair in presenting opposing views on doubtful matters, and so richly documented with bibliographical data (some items as late as 1934), that it must be regarded as the standard work of detail in its field.   On the other hand, it will not be an easy work for the tyro to consult (despite unusual care in explaining technical matters at their first appearance), for the extreme compression in presentation, the crowding of the typography, the long paragraphs, make difficulties except for the experienced.

I would now present a series of observations on single points.   17.5: 'One must seek to make intelligible the apparent exceptions to regularity of phonetic development': a vital point, somewhat neglected by those scholars who of recent years have sought to throw aside the principle of the phonetic law.   22.8: 'Many differences in speech start in mere errors which for some reason or other become general': well taken, but probably there has been in most instances the unconscious influence of some other word, now no longer identifiable.   24: Excellent remarks on the prerequisites of the worker in historical linguistics; cf. also page 45.   47 end: Schwyzer here and elsewhere shows that he shares my own high esteem of Meillet's Aperçu d'une Histoire de la Langue Grecque.   50 ftn. 4: On the date of Zoroaster, one might add Oriental Studies in Honour of Cursetji Erachji Pavry (London, Oxford Press 1933), which contains four treatments of the topic, by E. Herzfeld, C. F. Lehmann-Haupt, A. T. Olmstead, and myself.   51.6: Meillet's Introduction à l'Étude Comparative des Langues Indo-européennes is quoted in the sixth edition, 1924; but there is now a seventh, dated 1934.   54: The *centum* and *satem* division is here given as a rule of thumb, quite properly, and not as a genetic division, which cannot be maintained. 75–7: The attitude toward dialects is notably sane.

102 ftn. 2 (also 104–5 and 243 Zu tz 1): Schwyzer quotes, with apparent approval, Meillet's view that in the oldest writing two identical vowels not separated by another letter, might be reduced in writing to one, though each of the two represented a syllable; it is regrettable that this view persists, and I am glad to see that Schwyzer himself calls attention (102 ftn. 2) to the fact that it is not supported by epigraphic evidence.   139: Schwyzer accepts, I am glad to see, the view that the Greek alphabet is of Phoenician-Canaanitish origin; this can be supported by a curious shift in the names of the characters for the sibilants, which I have never seen in print: Greek *zeta* has the alphabetic position and the approximate sound of Semitic *zayin*, but the name of *tsade* in all except perhaps the initial sound; *san* has the position and sound of

tsade, and the name of *zayin*, unless it also keeps the initial of its proper prototype; *xi* and *sigma* bear precisely similar relations to *samekh* and *sin* (*shin*) respectively.   Such a shift is infinitely easier to explain on the current basis of Semitic origin for the Greek alphabet, than on any other.   143: Schwyzer regards Greek Y (V) as derived from Semitic *vau*: this seems to me difficult.   145–6: Schwyzer emphasizes the fact that the differentiation of E and H, O and Ω in writing was originally to express difference in quality, and not difference in quantity (though that also existed): good.   153: On Iranian names in Greek, add A. H. M. Stonecipher, Graeco-Persian Names (Vanderbilt Oriental Series, vol. 9; New York, American Book Co. 1918); for 'Ἰνταφρένης read -φέρνης—the Old Persian was certainly, as Schwyzer quotes it, *Vi(n)dafarnā*.   157.13: Quite probably the Old Latin Z was not entirely lost, but remained in Latin G; cf. G. Hempl, TAPA 30.24–41.   157.15 from the bottom: It is not likely that 'Latin *c* for Greek *κ* in transcriptions, and vice versa, was essentially orthographic', until quite late, for the *c* in such a word as *Cicero* was not assibilated until about the seventh century.   174: The bibliography on pronunciation lacks the extremely important work of E. H. Sturtevant, The Pronunciation of Greek and Latin (Chicago, Univ. of Chicago Press 1920).   194 ftn. 3: The writings with the macron in the first syllables of *Āiāx Trōia Nāias* are misleading, even though the proper pronunciation (*ai- įās* etc.) is given immediately thereafter.

210 bottom: Schwyzer does well in calling attention to the possibility of pronouncing as an aspirate the prior consonant in φθ and χθ as well as the latter.   235 ff.: Good on syllabification.   243 Zus. 1: I fail to see why there should be objection to the formula that H and Ω developed rather than the spurious diphthongs, before *e*-vowels.   245.22: Ital. *moglie* is not from Vulgar Latin (acc.) *muliérem*, but from nom. *múlier*; cf. C. H. Grandgent, From Latin to Italian §162.   247.3 from bottom: Another example of a *u*-diphthong arising by contraction, is seen in most forms of οὗτος, especially in those beginning with *ταυ-*.   282 Zus. 1: Note the full discussion of alternative views here, as indeed frequently.   287–8: In the Aeolic forms like *τοὶς* from *τους*, the *-ι-* may denote a nasalized pronunciation, as it certainly does in the (dissyllabic) *κοί* of the Megarian's daughters who were pretending to be little pigs, Aristophanes, Ach. 780.   295.6–7: The relation of βούλομαι to βάλλω, given here, is new to me and attractive.   300: On Aeolic labial stops for IE labio-velars before palatal vowels, see now LANG. 11.208–9.   305.32:

If ἡμέρα gets its rough breathing from ἑσπέρα, from what source does that word get it (cf. 226–7)? I suggest ἕως as the probable origin. 307.6: On the special development of IE dental groups, Emeneau in LANG. 9.232–6 is cited; LANG. 8.18–26, with an interpretation of the phenomena, is not cited.

330: On the representation of IE ị by Greek ζ or the rough breathing, I regret not to see a reference to E. W. Fay, CQ 9.104–14. 333.37: Μεγάβυξος is normally -βυζος. 334: I fail to see any reason to interpret the variable initial s before a consonant, as a prefix rather than as the product of wrong division; cf. the redivision of syllables in Greek words shown by writings with medial -σστ- etc., 238 mid. 336 Zus. 3: Schwyzer takes ἐς as a development of ἐξ before κ as impossible in dialects which had ἐς from ἐνς: I should rather interpret the phenomena as indicating that where the form ἐς from ἐξ was not eliminated by extension of ἐκ, the form ἐς for ἐνς (from ἐν by analogy of ἐξ) could not arise, and the original ἐν remained, governing accusative as well as dative. 340–1: The theory of a shwa-vowel still seems to me more reasonable, despite θετός, δοτός etc. 353: On ablaut, add Gray's two other articles, LANG. 6.229–52 (1930) and BSLP 31.2.34–42 (1931). 359 Zus. 2: On the equivalence Ar.ā = IE ŏ, Buck, AJP 17.445–72 (the essential article) might have been mentioned, and not merely included under a reference to Hirt, IG 2.19, for bibliography. 390.7: For Skt. dadhǎmi, read dádhāmi; but the example would then have to be omitted, as not serving the purpose for which it is quoted.

But these are details: if scholars could all agree on all details, learning would die of stagnation. Schwyzer's Griechische Grammatik is a veritable encyclopaedia on the subject, deserving to be kept at hand for consultation by every scholar working in the field; but when the second part, containing the morphology, has appeared, let us hope that it will contain an elaborate set of indexes which will make it possible for the user to find readily any material that he needs. The value of the volume will be at least tripled by such indexes.

ROLAND G. KENT

L'Aoriste Grec en θην. Pp. 224. By A. PRÉVOT. (Collection Linguistique publié par la Société de Linguistique de Paris.—XXXIX.) Paris: Librairie Ancienne Honoré Champion, 1935.

The author conceives this study as in the field of aspect in the Greek verb; he has been stimulated by ideas expressed by Meillet, and sup-

ports his views also on conclusions reached in volumes and articles by Chantraine (cf., for example, the review of the latter's Parfait Grec in this journal, 3.202–4).

He takes up and studies the examples, especially in Homer, with continuations in later authors, of those verbs which have aorist passives of two forms, in -ην and in -θην. He shows, quite conclusively, that where a distinction can be felt the -ην forms were old aorist forms, used absolutely in the general value of a state or condition, and changed in Greek at a very early date to a passive of state or condition ('indéterminé', almost the same as imperfective); and that the forms in -θην were a newer formation to denote past action, passive (déterminé, almost perfective). The former, he shows, was an older inherited formation, which was never truly living in Greek: there are 129 verbs with -θην forms in Homer, and only 25 with -ην forms. The newer form was convenient because it could be added to vowel stems without hiatus, and in its special meaning filled a gap in the verbal equipment as it was being built up in Greek into the beautiful system which Greek ultimately employed. The semantic relations thus supplied, in comparison with the s-aorist forms (active and middle), the second aorist and the second perfect forms (active and middle), are set forth with clarity and precision. The investigation is brought down as far as Hellenistic times, with an occasional example of later date.

Two points may here be touched upon. In a summing up (112), Prévot says that 'to a transitive aorist in -σα (or middle -σάμην) which draws attention to the object of the verb, Greek set up a (passive) aorist in -θην which draws attention to the subject.' The evidence lies in the fact that the one commonly has the object expressed and the other rarely has the agent expressed. But is this not the proper function of any passive form qua passive?

The other point is the origin of the -θη- suffix. He cites (21–2) the view advanced by Wackernagel, that it is an extension of the second singular personal ending -thēs, seen in ἐδόθης = Skt. ádithās, etc., but dismisses this as unlikely; he cites Meillet's view that it may contain the same element as in the present formations σχέθω, τελέθω, φθινύθω as compared with the presents ἔχω, τελέω, φθίνω, and (94) accepts this view. But this is only ignotum per ignotius erklären; and he never mentions the fact that there is another view associating this suffix with the dental ending of the Germanic preterite (Brugmann, Kl. vgl. Gram. d. idg. Spr. 2.550). According to this view, the second part of the -θην

passive is an aorist $*\check{\epsilon}\theta\eta\nu$ to $\tau\acute{\iota}\theta\eta\mu\iota$, with the long vowel extended to all numbers by the analogy of the aorist in -$\eta\nu$, in which the vowel did not vary. Though Thumb (in his revised fourth edition of Brugmann's Griech. Gram. 382) rejected this view, and Buck (Comp. Gram. of Greek and Latin 285) and Meillet-Vendryes (Gram. comp. d. langues class. 215) do not even mention it, it is still worthy of consideration, as it is approved by Hirt (Hdb. d. gr. Laut- und Formenlehre[2] 558; IG 4.98), and might easily be brought into harmony with Prévot's conclusions as to the semantic value of the form.

The value of Prévot's work lies, however, in the keen analysis of the examples, the establishment of the precise semantic value of the forms, the commentary on the historical development. Unfortunately details of this kind hardly lend themselves to suitable treatment in the limited space of a review; but the conclusions, in their essentials, have been stated above.

ROLAND G. KENT

Sprache und Stil der Iamata von Epidauros. Pp. vi + 92. By RUDOLF NEHRBASS. (Philologus, Supplementband 27, Heft 4.) Leipzig: Dieterich'sche Verlagsbuchhandlung, 1935.

Professor Rudolf Herzog, in Die Wunderheilungen von Epidauros (Philol., Suppl. 22.3), gave a full discussion of the text and content of the four great inscriptions set up at Epidauros in the second half of the fourth century before Christ, containing edited versions of the miraculous cures which had there been wrought. The texts are in a uniform, semi-literary style, and can therefore be judged as a unit.

Dr. Nehrbass, as a pupil of Professor Herzog, took the language of these inscriptions for the subject of his doctoral dissertation, which, with later additions, is here presented to us. He gives in detail the phonology and the morphology, so far as there are differences from the Argive Doric or from the Koine, showing that the language is essentially Argive, with a few variations in the direction of the Koine. The second chapter deals with the syntax, the third with the style; I judge that the last chapter is the most valuable, since his listings show the precise form or forms taken by each phrase in the make-up of the inscriptions, and enable other scholars, in studying mutilated inscriptions of the same character, to see at once what possibilities exist for each gap. The work is detailed, but, as might be expected, brings no startling conclusions.

ROLAND G. KENT

Thesaurus Linguae Latinae Epigraphicae; the Olcott Dictionary of the Latin Inscriptions. Volume II, Fascicle 1 (Asturica-audio). Pp. 24. By Leslie F. Smith, John H. McLean, Clinton W. Keyes. New York: Columbia University Press, 1935.

Professor George N. Olcott of Columbia University died in 1912, having published twenty-two fascicles (520 pages, A-Asturia) of his Dictionary of the Latin Inscriptions. After a lapse of more than twenty years, the means have been found to continue the work, and members of the Columbia University Faculty are engaged upon the enterprise; Mrs. Olcott has made available to them the materials which her husband had gathered for later volumes.

The work is to be continued along the lines laid down by Professor Olcott, and it will, when completed, be an invaluable tool for the Latinist, as an exhaustive dictionary of the vocabulary of the Latin inscriptions. The present fascicle contains a number of important words: *at* and *atque, ater* and *atrium, auctor* and *auctoritas, audeo* and just the beginning of *audio*. I am a little surprised that under *Atalanta* the Praenestine *Ateleta* is cited according to the first edition of CIL I, while forms of *audio* are cited according to the second; on *Ateleta* the second edition has some valuable data lacking in the first.

We are now in a position to study satisfactorily the sandhi forms of *ad* from the inscriptions; also, we find that the final syllable of *Atropos* was never Latinized in them; and with the opening page of the next fascicle, we shall have a complete inscriptional record of the presence and the absence of the *v* in the perfect forms of *audio*. These, and many other topics in the history of the language, can be studied satisfactorily only from a complete listing of inscriptional occurrences, such as the Thesaurus Linguae Latinae Epigraphicae gives us, or will eventually give us.

<div align="right">Roland G. Kent</div>

A History of the English Language. Pp. xiv + 509. By Albert C. Baugh. New York and London: D. Appleton-Century Co., 1935.

The volume represents the ripened fruit of Baugh's broad and precise scholarship, presented in a form that is at once attractive and authoritative. There is scarce a paragraph in which we do not see the evidence that while he has utilized to the full the secondary literature, he is master of all the original sources and has often come to conclusions which are distinctly his own.

After chapters which serve as an introduction to linguistic study in general and to that of an Indo-European language in particular, he

carries his history of English from the earliest records of speech of England and from the first evidence for the speech of those tribes who brought Teutonic dialects into the island of Britain, down to the present-day speech of England and of the United States, with attention to the English used in the British Dominions and Colonies as well. Special features are the following:

1. A detailed exposition is given of the relations of French and English from the Norman Conquest, and even before that date, down to the final victory of English; documented by brief pertinent citations from the original sources—as indeed are all portions of the book, yet in such a way that they blend with the narrative instead of making it disjointed.

2. In the chapter on the Renaissance, 1500–1650, he shows that English, precisely like French and Spanish, faced at that time three great problems: recognition in fields where Latin had held sway up to that time; the establishment of a more nearly uniform orthography; the enrichment of the vocabulary to meet the widening demands upon it.

3. Somewhere in the chapter on the Renaissance, Baugh reaches the point at which other comparable histories of our language have stopped; but he goes on with chapters on The Appeal to Authority, 1650–1800; The Nineteenth Century and After; The English Language in America— and I wish that every Anglophobe in our country and every Americophobe in England could read his sane treatment of this last topic.

For the scholar's benefit every chapter is provided with a well-selected general bibliography (some items as recent as 1934), as well as with footnotes; the non-technical reader can ignore them without trouble. I add here a few selected jottings by way of correction or difference of views:

Page 21.27: 'The most important discovery leading to this hypothesis (that the languages of a large part of Europe and part of Asia were at one time identical) was the recognition that Sanskrit was one of the languages of the group': rather, it was acquaintance with Sanskrit which made possible the formulation of the hypothesis. 28.10: The amount of the Arabic element in Persian is here somewhat overestimated. 32.10: Venetic is now thought not to be an offshoot of Illyrian. 34.22: Between the use of *urbs* and that of *villa* for 'city', there intervened the use of *civitas* (French *cité*, Ital. *città*, now in specialized meanings). 35.24: Lithuanian is spoken by many who are outside the boundaries of Lithuania, notably in Poland and in the United States. 37.4: 'Czecho-Slovakian' is not a single language, but two languages, each with its own literature; although the Austrian census of 1910 did not distinguish them, the Republican Government's census in 1921 gives 6,792,954

speakers of Czechish and 1,967,983 speakers of Slovak.   38.33: The
Danish which is the written language of Norway has in fact some
important differences from that of Denmark.   42.8: Most scholars
recognize Hittite as the earliest offshoot from the Indo-European, and
it is desirable to have one term for the pre-Indo-European-Hittite, like
Sturtevant's Indo-Hittite, and not to use Indo-European to designate
the parent stock both before Hittite had split off, and after the split
as well.   42.15: Not merely 'some of the earliest records', but the
earliest records without qualification, of any Indo-European (= Indo-
Hittite) language are found in the earliest Hittite documents.   68.2
from bottom: 'When the stem is short' would be clearer if 'stem-
syllable' were used.

93.2: 'Latin *ĭ* changed to *ę* before 400 A.D.'; no, only *ĭ* changed to *ę*,
while *ī* remained.   132.31: The name of the Norman capital should
be given here.   197.24–7: Unfortunately phrased, since it sounds as if
Chaucer came after the close of the Middle English period.   199.3–7:
How could the modern pronoun *she* come from Old English *hēo*, except
by the influence of the demonstrative *sēo*?   211.28–30: 'It is melancholy
to think what the English dinner table would have been like, had there
been no Norman Conquest': rather unfair, since there would have been
other dishes and viands, even if the names had not been French in origin.
216.12: 'Central French showed an early distaste for the *w-* sound':
rather, the speakers of Central French had no *w* in their own speech, and
when they borrowed Germanic words containing it, they made a substi-
tution (*gu*) which was easier to them.   225.19: The abstract *-red*
survives also in *hund-red*.   227.6: *Face* is a 'common part of the body'
for which there is no word of English origin in use.   231.30: 'fighting
with' is ambiguous; it here means 'fighting in the ranks of'.   232.8: The
rare word *hanse*, unglossed (cf. §169), will baffle many readers.   275.11:
Epitome is not the classical Latin form, but the Greek form which in
late Latin was substituted for *epitoma* (used by Cicero).   275.19: It is
somewhat misleading to say that 'the Latin ending *-us* in adjectives
was changed to *-ous* (*conspicu-us* > *conspicuous*)', because *-ous* was the
development of *-osus*, which had a wide expansion in late Latin, and
not the product of *-us* by phonetic change; the same is true, mutatis
mutandis, of the ending *-al*.   275.32–6: The use of the Latin past
participle as the basis of an English verb is not an innovation in English,
but an old Latin practice, which was extended in later Latin and in the
derived languages; thus *repel* and *repulse* go back to Latin *repellere* and
repulsare (the latter used by Lucretius), while *pellere* itself had a deriva-

tive *pultare* 'knock (on the door)', used by Plautus and Terence, but archaic even in their time, for which they used also *pulsare* 'beat, knock' (in all meanings). 282.36: The word *concinnity* is said not to have survived, but it does even now have a limited use (which is all that it could ever have had). 303.3: It should be added that *thou*-forms are still regular in prayer to the Deity. 302.25: This would be clearer if *who* were at this point called 'the original interrogative *who*'. 303.21-2: It is confusing to read that *who* in 'Who hath ears to hear, let him hear', is an indefinite pronoun; even in this sentence it would be better to call it an indefinite interrogative pronoun. 306.1-10: The third plural in -(*e*)*s*, like *teaches,* can be explained clearly to the non-technical reader by the proportion: *I teach : we teach = he teaches : they teaches.* It might be added that illiterate speech of today is inclined to say *they was,* while the hyper-correction of the illiterate uses *he were.* 373.18: The use of the word *prohibition* to mean 'total abstinence from alcoholic beverages'[1] developed long before 1910, for there was a National Prohibition party formed in the United States in 1869. 374.11: While *zither* comes into English from German, it was taken by German from Latin, and by Latin from Greek. 393.30: *Fash* should not be glossed 'trouble oneself', but merely 'trouble', since it requires the object pronoun to be expressed.

But these points, if flaws at all, are mere trifles in view of the importance of the work; I regard Baugh's History of the English Language as worthy to take a place with the other great histories of single languages, of which there are regrettably few, but at the head of which I set the third edition of Meillet's Aperçu d'une Histoire de la Langue Grecque.

<div align="right">ROLAND G. KENT</div>

Semitic and Hamitic Origins, Social and Religious. By GEORGE AARON BARTON. Pp. xvi + 395, Philadelphia: University of Pennsylvania, Press, and London: Oxford University Press, 1934.

This work deals with the same topics as those treated in the author's former work A Sketch of Semitic Origins (1902), but as he states himself in his preface with an almost complete change of attitude towards them.

The work is divided into ten chapters. Chapter I deals with the questions of the cradle lands of the Semites and Hamites and of the relation of these two groups to one another; Chapter II discusses the neighboring peoples; Chapter III, the composition of the Semitic and

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[1] [I do not know the word in this meaning, but only in that of 'prohibiting (to others) the use of alcohol.' GMB.]

Hamitic peoples; Chapter IV treats early Semitic and Hamitic social life; Chapter V presents a general discussion of the religious origins of these peoples; the remaining chapters (VI–X) treat in more detail the religious origins of specific groups, Egyptian, South Semitic, Babylonian, West Semitic, Hebrew. The text of the book is followed by an Epilogue in which the author outlines (357–60) what he feels has been accomplished in his work; an Additional Note on a third poetical text from Ras Shamra (361–64); some Addenda dealing with causative stems, languages of Asia Minor, *Ahhiyawa* in Asia Minor, Semitic and Egyptian vocabularies, levirate among the Hittites, menhirs and gilgals, circumcision, *nûn* 'fish', the tent of meeting, the god Anu (365–9); an Index of Scripture References (371–4); an Index of Authors (375–81); and an Index of Subjects (383–95). Useful tables of Hamitic and Semitic pronouns and verbs are enclosed in the back-cover pocket.

In Chapter I the author presents a summary of the various theories with regard to the primitive homes of the Semites and Hamites, and decides that in view of all the evidence available both peoples originated most probably in North Africa, whence the Semites migrated to Arabia, which became their original Asiatic home land. This view accords well with the old theory now recently supported by Meinhof that Semitic and Hamitic are related to the negro languages of the Sudan, and perhaps to the Bantu languages of South Africa (cf. C. Meinhof, Das Ful in seiner Bedeutung für die Sprachen der Hamiten, Semiten und Bantu, ZDMG 65.177–220 [1911]). In the second part of the chapter the author discusses the question of the relation between Hamites and Semites. He concludes that the two peoples were originally one, and that the Semites migrated from Africa to Arabia.

There are a number of points in the work which may be criticised from a linguistic point of view, as the discussion of linguistic matters is not always as clear cut or as scientific as might be desired. Note the following: 'pronouns are among the most *sui generis* of the parts of speech' (12, 21); 'another common trait of the Hamitic languages is the elision of a vowel in the body of a word, and the modification of the following consonant in consequence' (13, what this means is not clear, as no examples are given); 'the pressure tone ' , and its voiceless counterpart h' (18, should be h, or is the h a misprint?); 'stems expressing causation are formed by prefixing the letter (!) s, tho in some of the Semitic languages this is thinned (?) to h' (22); 'n has become m by a change familiar to philologists' (24); 'the fact that n has been so often changed to m and r, and that the meaning of the stems have assumed such a

variety of meanings (*sic*), are guarantees of the high antiquity of the *n* formations' (24, *non sequitur!*); 'the *im-im* became in time Mermer. This gave rise to the variants Immer and Inimer' (246, 247, how?); 'Zarpanitum . . . means "Creator of seed"' (273, *Zer-banitum*, which has this meaning, seems to be a popular etymology for the similar sounding *çarpanitum* 'silvery', originally an epithet of the deity, afterwards her name; cf. Muss-Arnolt, Assyrisch-Englisch-Deutsches Handwörterbuch, Berlin, 1905, 2.894 f.; Delitzsch, Assyrisches Handwörterbuch, Leipzig, 1896, 574); the connection of Hebrew *am* 'people' with *am* 'with' (208) seems very problematic in the face of Hebrew *immādî* 'with me', Arabic *inda* 'with'. After praising and apparently accepting Speiser's theory of the causative prefixes *š* and *h*, the author says (367): 'it still seems to the writer that the theory that the *h*-forms were derived from the *s*-forms by a phonetic change, which did not prevail where these letters (!) occurred in other parts of speech, and which is accordingly an exception to ordinary linguistic rules, is simpler and more probable than any other'. The influence of one language or group of languages upon the morphology of another, an influence that is very difficult to define and prove is taken as a matter of course (22). The judgment 'failure' pronounced upon the work on Indo-European-Semitic relations of H. Möller seems too severe a sentence to pass on an investigation, which, while it can not be said to have proved its case beyond cavil, certainly, to say the least, marks a decided advance over past efforts along this line (cf. H. Möller, Vergleichendes indogermanisch-semitisches Wörterbuch, Göttingen, 1911; F. R. Blake, 'The Relation between Indo-European and Semitic' in Oriental Studies in Honour of Dasturji Saheb Cursetji Erachji Pavry 41–8, Oxford University Press, 1934). The theory of the Amorite origin of the Alphabet (75) does not seem to be sufficiently supported by the evidence. In discussing the relationship between Akkadian and South Arabic, use might have been made of the excellent summary of the similarities between Ethiopic and Assyrian by the late Professor P. Haupt (cf. Prolegomena to a Comparative Assyrian Grammar, PAOS, Oct. 1887, ccxlix ff.)

Thruout the volume the great archaeological erudition of Professor Barton is plainly evident, and his possession of the spirit of genuine scholarship is shown not only by the reserve with which he states his opinions, but also by his willingness to change his position on any subject when such a change is indicated by the advance of knowledge. The author has assembled here in this work a full store-house of material for the 'origins' with which he deals, especially for Egyptian and Semitic

religious origins, and the material is so arranged that it is available for use, whether the reader agrees or differs with the author's explanations or conclusions.

<div align="right">FRANK R. BLAKE</div>

Arabia and the Bible. Pp. x +207. By JAMES A. MONTGOMERY. Philadelphia: University of Pennsylvania Press, 1934.

This book, which discusses at length the relations between Palestine and its Hinterland, Arabia, is divided into eight chapters (1–188), preceded by a *Preface* containing the meager bibliography of the subject, and followed by Indexes including (189–194) an Index of Scripture References, arranged under the individual books. The inside of each cover, front and back contains a good outline map of the Arabian peninsula.

The first chapter points out the special necessity of considering Arabian influences in any discussion of Palestine or the Bible, and emphasizes the essentially Arabian character of Hebrew culture. In the second chapter all the Biblical occurrences of the terms 'Arab', 'Arabian', 'Arabia' are discussed, the chapter opening with an investigation of the etymology of the Hebrew correspondents of these words. The third chapter treats the kinship of the Hebrews and the Arabian peoples, as revealed in the various Biblical genealogies. Chapter IV collects all ancient extra-Biblical references to Arabia, Assyro-Babylonian, Egyptian, and Greek. Chapter V deals with the geography, physical and political of 'Arabia Deserta', stressing the fact that Arabia, instead of being a desert waste inhabited sparsely by semi-savages, has always been the center of a vigorous and individual life and culture developed by a proud, virile, and able race. Chapter VI deals with the 'remarkable civilization which flourished as far back as the beginning of the first millennium B. C.' in Yemen or South-West Arabia. Chapter VII deals with the relations between South Arabia and the Bible, which are studied thru the medium of South Arabian names and South Arabian religious institutions, numerous linguistic correspondences, morphological and lexical, being pointed out. The chief results of the investigation are summed up in the eighth and concluding chapter.

There are a few minor points in the book that offer ground for criticism. There are very few misprints. Occasionally an indicated reference is omitted or given only in general terms. The use of *h* for both *He* and *Ḥeth,* and of *s* for both *Çade* and *Samech,* in the transliteration of Semitic words, occasionally leads to ambiguity and confusion,

e.g., *Hasor* for *Ḥaçor* (64), *s* for *ç* in *Salm, Sanam,* and *Sur,* for *s* in *Semel* (67). On page 30 *Jethro* is cited in connection with *Gashmu* as a good Arabic form, but no explanation is given of the final vowel *o*, presumably for -*u*. On page 38 *Yoktan* is explained as an imperfect form, properly *Yektan*, but no comment is made on the *o*; *Yoktan* may perhaps represent an imperfect passive form like *ịuttan, ịuqqaḥ, ịûkal.* The interesting explanation given of the name Abraham (167) may be correct; the *h*, however, may be similar to the secondary *h* in Syriac *behet* (Hebr. *bôš*), *reheṭ*, (Hebr. *rûç*), Hebrew *'amāhôt*, Arabic *ummahât*, Aramaic *abâhât*.

The work as a whole presents a most sane and carefully reasoned outline of the part played by Arabia in the history and development of its neighbors, particularly the Hebrews, and furnishes a convenient and valuable handbook on Arabia for all those interested in things Biblical.

FRANK R. BLAKE

Petit Précis de Grammaire Chinoise écrite. Pp. 64. By GEORGES MARGOULIÈS. Paris: Adrien-Maisonneuve, 1934.

This brochure has as its aim to interpret the syntactical relations of the current written Chinese language, by a comparison with semantically equivalent French. This is the only proper way to get an understanding of a fundamentally different language, though it leads to non-idiomatic (though comprehensible) expressions in the language used for interpretation.

Chinese, says the author, has a very strict system of syntax, which is indicated by particles or 'empty' words, by parallelism of phrases, by rhythmical equivalence of phrases: the most important point is to identify the beginnings and the endings of sentences and phrases, which are marked by the empty words. He gives then a list of 69 empty words with their functions, and a sample sentence interpreted in the light of the empty words.

His exposition is quite illuminating, as it is intended for beginners in Chinese; but he apparently falls into the error of assuming that the Chinese feel the difference between noun and verb, whereas in reality they feel no such difference.

The brochure is mechanically reproduced from typewritten pages, written on a machine with keys not clean and badly aligned. The spacing is also bad. These defects might have been avoided.

ROLAND G. KENT

Textes Populaires Inguš, recueillis par M. JABAGI; traduits, commentés et précédés d'une introduction grammaticale par G. DUMÉ-ZIL. Pp. 75. Paris: Adrien-Maisonneuve, 1935.

The Inguš is a north-central language of the Caucasus, closest akin to Čečen and to Bac; previous studies are mostly in Russian, except Dumézil's article on the verbal conjugation in BSLP 34.1.97–114 and a brief description by Dirr in his Einführung in das Studium der kaukasischen Sprachen 143–8. The present volume, reproduced from autograph copy, contains a brief summary of the grammar, followed by the text of seventeen ballads, similar in tone and subject-matter to the Lithuanian *dainos*: they bear on war, love, sorrow. These ballads had been learned by Jabagi in his youth, and recently, in exile in Paris, he placed the text at the disposal of Dumézil, who had already made studies in the language. The text is accompanied by a literal interlinear translation, a free translation, and a commentary on the word forms and uses.

Scme outstanding features of Inguš are the following: Most nouns (but not all) take class prefixes, often altered for the plural, which are prefixed also to their adjective modifiers and to their predicate verbs. The case system is elaborate, of twelve forms; the plural has a plural element (except in the genitive), but phonetic combination with the case-ending often makes the relation to the endings of the singular obscure. The verbal inflection is characterized by changes in the radical vowel, mostly or exclusively resulting from front and back umlauts due to the vowels of the terminations, which have now been, in most instances, reduced to the obscure vowel or entirely lost. As in Slavonic, aspect is an important feature in verbs. There is no transitive verb; 'you beat me', with an 'operative' verb, is expressed by 'I am-beaten by-you', the 'by-you' being in the 'ergative' case, and 'I see you', with an 'affective' verb, is expressed by 'I am-in-sight to-you', the 'to-you' being in the dative.

While none of these features is unfamiliar in Caucasic languages, the present volume, with its detailed exposition of the texts, enables the student to get a clear idea of them in their practical working.

ROLAND G. KENT

La Langue Gauloise Resuscitée. Pp. xii + 188. By MICHEL HONNORAT. Paris: Librairie Ernest Leroux, 1935.

The author in 1933 published two volumes, one demonstrating the kinship of the Indo-European and the Semitic languages, and another showing the kinship of Chinese with the Japhetic, Semitic, and Hamitic

languages. He now seeks to show that French is derived from Celtic, and gives 112 pages of Breton-French equations, followed by 36 pages of Welsh-French equations (stopping early in D). His conclusion (ix) is that 'ninety percent of French words, ancient and modern, are Celtic words, which are found in almost identical forms in the six Celtic dialects'. It is not worth while to go into detail here.

<div align="right">ROLAND G. KENT</div>

L'Énigme Lydienne (les inscriptions de Sardes). Pp. 51. By F. BUTAVAND. Paris: Adrien-Maisonneuve, 1935.

This is a series of studies of the Lydian inscriptions which were published by Buckler, in 1924, as the fruits of the American excavations at Sardis. Butavand's conclusion is that despite a very ancient linguistic remnant which is neither Indo-European nor Ural-Altaic, the Lydian language is closely allied to the 'groupe gréco-latin', etymological correspondences with Greek and Latin being listed (35–9). Also, he finds that a number of the inscriptions, notably No. 13 (Buckler's numbering), translate passages of the Avesta.

Scholars no longer admit the close grouping of Greek and Latin which Butavand assumes; and when I note that the same author has interpreted parts of the Etruscan text on the mummy-wrappings of Agram as fragments of the Odyssey (Revue de Philol., de Litt. et d'Hist. anc., 1926–7), I become skeptical both of his competence and of his results.

<div align="right">ROLAND G. KENT</div>

NOTES AND PERSONALIA

LANGUAGE goes on a new schedule in 1936. Each issue will correspond to one quarter of the calendar year, and will be distributed, if possible, about the middle of that quarter: thus the present issue will be dated January-March, and will be issued before February 15, provided printer, editor, and proof-reading authors do their respective duties promptly. It is here called to the attention of the recipients of the publications that the third issue will be scheduled for distribution in August, and it is incumbent upon them to make arrangements for the holding or forwarding of the issue, if they are then away from their regular addresses; otherwise the issue will be returned to the Society's office and postage collected, a letter will have to be written to find out the whereabouts of the member, and added postage used for the resending of the issue. Members are urgently requested to save for the SOCIETY this expenditure of money, and for the Secretary this extra labor.

Another innovation will be that the Proceedings of the Annual Meeting and the List of Members, perhaps with other business matters, will be issued in a separate BULLETIN; this leaves LANGUAGE free to carry scientific matter only, except for Notes and Personalia, and the list of Books Received. The BULLETIN, it is hoped, will go out with the first issue of LANGUAGE; but circumstances may delay it until the appearance of the second issue.

LOUIS ALLEN, Associate Professor of French in University College, University of Toronto, and a member of the LINGUISTIC SOCIETY OF AMERICA since 1926, died suddenly at Béziers, France, on August 27, 1935.

He was born in Penetang, Ontario, but was brought up in the United States; his parents still reside at Clinton, Illinois. He received the B.A. and the M.A. from the University of Illinois, and the Ph.D. from the University of Chicago. For a time he taught at the University of Oklahoma, after which he was called to Toronto, where he spent the remainder of his life. In June of 1935 he sailed for Europe with Mrs. Allen, to visit Soviet Russia. They returned by way of Vienna, to Mrs. Allen's former home in Béziers, where his death occurred.

Professor Allen was an ardent linguistic scholar, and possessed an

exceptional knowledge of Late Latin and Old French; he published various mediaeval studies, the last of which, entitled La Vie de Saint Johan Paulus, appeared just before his departure for Russia. He also studied American Indian languages, spending many vacations in their pursuit; a number of articles were the fruit of these studies.

Though somewhat reticent by habit, he was extremely friendly by nature and talked freely and entertainingly when the right kind of company tapped his interests. He was one of a group that ate regularly at the Faculty Union, at the same table, which suggests that he was one of those who prefer a few good friends of long standing, to a multitude of mere acquaintances.

NORMAN W. DeWITT

BRYN MAWR COLLEGE commemorated on November 1 and 2, 1935, the Fiftieth Anniversary of its opening, which took place on October 23, 1885. Among the delegates were several members of the Linguistic Society: Lily Ross Taylor, representing the American Philological Association and the American Academy in Rome; Edwin C. Armstrong, representing the Modern Language Association of America; George A. Barton, representing the Philadelphia Episcopal Divinity School; and Roland G. Kent, representing the American Oriental Society.

E. A. SPEISER, Professor of Semitics at the University of Pennsylvania, reports what may be a record-breaking class in Hittite, at least for the United States; he has ten students, of whom nine have previously studied the cuneiform system of writing and six have had three years of Accadian. The class uses Sturtevant and Bechtel's Hittite Chrestomathy, Sturtevant's Comparative Grammar of the Hittite Language, and Sturtevant's Hittite Glossary, all printed by the Linguistic Society.

Ethel G. Aginsky is pursuing linguistic researches among the Indian tribes of the Western States.

Lawrence S. Hitchcock, Headmaster of the Los Alamos Ranch School, Otowi, N. M., is spending the present academic year in graduate study at Yale University.

George A. Meyer, formerly Instructor in French at Yale University, has gone to the State Agricultural College at Logan, Utah, as Professor of Modern Languages, and Head of the Department.

George Nordmeyer, formerly of the Department of German at Yale University, has gone to the West Virginia University as Instructor in German.

THE FOLLOWING NEW MEMBERS FOR 1935 were received into the LINGUISTIC SOCIETY subsequent to the last published list, and up to November 6, 1934:

Joseph M. Carrière, Department of Romance Languages, Northwestern University, Evanston, Illinois.

Miss Leonora Frances Curtin, Acequia Madre, Sante Fé, New Mexico.

Dr. Atsuo Kobayashi, Faculty of Letters, Tōhoku Imperial University, Sendai, Japan.

Benjamin Schwartz, M.A., 338 East 15th St., New York City.

Benjamin Weiner, B.A., 94 West Third St., Bayonne, N. J.

To the same date, the following new members for 1936 had been received:

T. R. S. Broughton, Ph.D., Associate Professor of Latin, Bryn Mawr College, Bryn Mawr, Pa.

Oscar E. Johnson, Ph.D., Associate in English, State University of Iowa; 1209 E. Devenport St., Iowa City, Iowa.

BOOKS RECEIVED

Under this heading will be acknowledged such works as seem to bear on the advancement of the scientific study of language.

The publicity thus given is regarded as a full return for the presentation of the work. Under no circumstances is it possible to comply with the requests being made by certain publishers for the return of books not reviewed quickly.

Reviews will be published as circumstances permit. Copies of them will be sent to the publishers of the works reviewed.

For further bibliographic information consult the annual list of Exchanges.

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Jahrhunderts. Pp. xvi + 290. By ELISE RICHTER. Halle/Saale: Max Niemeyer Verlag, 1934. (Zeitschr. f. rom. Phil., Beiheft 82.)

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LANGUAGE OR IDEAS?

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[The logicians of the Vienna Circle have independently reached the conclusion of *physicalism*: any scientifically meaningful statement reports a movement in space and time. This confirms the conclusion of A. P. Weiss and other American workers: the universe of science is a physical universe. This conclusion implies that statements about 'ideas' are to be translated into statements about speech-forms.]

Some years ago I had the honor of addressing the Linguistic Society of America and one of the sister societies upon a prescribed subject 'Linguistics as a science'.[1] The views which I was bound to express were shared by so few people that it seemed natural to state them in the form of prediction rather than of dogma. Linguistics as actually practised employs only such terms as are translatable into the language of physical and biological science; in this linguistics differs from nearly all other discussion of human affairs. Within the next generations mankind will learn that only such terms are usable in any science. The terminology in which at present we try to speak of human affairs—the terminology of 'consciousness', 'mind', 'perception', 'ideas', and so on—in sum, the terminology of mentalism and animism—will be discarded, much as we have discarded Ptolemaic astronomy, and will be replaced in minor part by physiological terms and in major part by terms of linguistics.

This prediction was based not only upon what seem to me to be the striking features of linguistic methodology, but in far greater measure upon the doctrine of non-animistic students of human behavior, especially upon the conclusions of our late colleague, Albert Paul Weiss.

A prophecy of this sort, no matter how deep the conviction from which it springs, is so pitifully subject to individual prejudices and errors that even more than most statements it needs to be confirmed or refuted. Within the last years a group of philosophers and logicians, known as the

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[1] Studies in philology 27.553 (1930). The summary which follows above is stated so as to bring out the accord with the Viennese conclusions (see below).

Vienna Circle, has arrived at the same conclusion concerning language.[2] Subjecting various branches of science to logical scrutiny, Rudolf Carnap and Otto Neurath have found that all scientifically meaningful statements are translatable into physical terms—that is, into statements about movements which can be observed and described in coordinates of space and time. Statements that are not made in these terms are either scientifically meaningless or else make sense only if they are translated into statements about language. The former, entirely meaningless type may be illustrated by the sentence: *The world is known to me only through my perceptions.* This statement is scientifically meaningless, for it directs us to no observation at any place or time; it predicts nothing.[3] The second type may be exemplified by the sentence: *Redness is a concept.* This makes sense only if it is translated into a statement about language, namely: *In the English language the word redness is a noun.*[4]

[2] R. Carnap, 'Ueberwindung der Metaphysik,' Erkenntnis 2.219 (1931); also in a French translation, which I have not seen, La science et la métaphysique, Paris 1934 (= Actualités scientifiques, vol. 172); 'Die physikalische Sprache,' Erkenntnis 2.432 (1931); also in an English translation, which I have not seen, The unity of science, London 1934 (= Psyche miniatures, General series, No. 63); 'Psychologie in physikalischer Sprache,' Erkenntnis 3.107 (1932); 'Les concepts psychologiques,' Revue de synthèse 10.45 (1935); Logische Syntax der Sprache, Vienna 1934 (= Schriften zur wissenschaftlichen Weltauffassung, 8); Philosophy and logical syntax, London 1935 (= Psyche miniatures, General series, No. 70).

O. Neurath, 'Physikalismus,' Scientia 50.297 (1931); 'Physicalism,' The monist 41.618 (1931); 'Soziologie und Physikalismus,' Erkenntnis 2.393 (1931); Einheits-wissenschaft und Psychologie, Vienna 1933 (= Einheitswissenschaft, Heft 1); Le développement du cercle de Vienne, Paris 1935 (= Actualités scientifiques, vol. 290).

[3] This example is modeled on Carnap's examples in Philosophy and Logical Syntax, 16ff.; for a thoroughgoing analysis see Weiss's article on solipsism, Psychological Review 38.474 (1931).

[4] Compare Carnap, 62, who uses *thing-word* for *noun*. The term *noun* (or *thing-word*), of course,—though Carnap does not mention this—must then be defined, for English grammar, and the term *word* for language in general, as technical terms of linguistics; this definition, moreover, must be made in terms of the postulates, undefined basic terms, and earlier definitions of linguistics—not by definitions of meaning and not in metaphysical terms. Thus, a *word* is the smallest meaningful unit that can be spoken alone. In English, a *noun* is a word which enters centrally into endocentric phrases with preceding adjective modifiers, serves as an actor with a finite verb, as the goal of a verb or preposition, and as a predicate complement, appears always in one of two sub-classes, singular and plural, and joins with the suffix [-ez, -z, -s] to form an adjective. Carnap, so far as I have found, nowhere mentions the fact that the discourse of logic presupposes descriptive

The path by which Carnap and Neurath reach this conclusion is thorny. It is the path of 'pure' formal logic, with abstraction from all empirical content. Carnap employs mentalistic terms and has to struggle with them; both Carnap and Neurath use linguistic terms without reference to their empiric background. These defects keep our authors from attaining to the mathematical elegance and cogency, the surgical precision, or the vast human scope of Weiss's 'Theoretical Basis', Yet their thornier path follows the same direction. A summary of their argument, given in non-mentalistic terms, could serve directly as a formal résumé of the steps by which Max F. Meyer or A. P. Weiss reach the same goal.[5]

Carnap and Neurath agree, then, with the American students in saying that mentalistic phraseology, in so far as it is not nonsensical, is only a troublesome duplication of linguistic phraseology. The most important feature of this agreement is the circumstance that Carnap and Neurath have done their work in complete independence of their American predecessors. They mention American work a very few times, and then in such manner as to guarantee their lack of familiarity with it. It is safe to say that we have here a highly significant confirmation; the Vienna authors, working independently and with a different method, have reached the same conclusion, stating it in terms which need not even be 'translated' to show the equivalence.[6]

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linguistics and uses the technical terms of this empirical science. The complex linguistic background of logical and mathematical statement is generally ignored by philosophers and logicians; an informal outline of it will be found in Philosophy of Science 2.499 (1935); more formally in my Language, New York 1933, chapters 2 to 16.

[5] Max F. Meyer, The Psychology of the Other One, second edition, Columbia, Missouri, 1922.

Albert Paul Weiss, A Theoretical Basis of Human Behavior, second edition, Columbus, 1929.

[6] In Der logische Aufbau der Welt (Berlin 1928) 81, Carnap mentions Watson and, of all people, Dewey, as behaviorists; in Erkenntnis 3.124 (1932) he mentions a German translation (1930) of Watson's Behaviorism. Neurath, Einheitswissenschaft 20, analyzes a paragraph of this German translation and finds that Watson's use of the terms *good* and *bad* violates the rule of physicalism; from this, Neurath seems to draw the conclusion that Watson and all other American students fail to satisfy the demand of physicalism. As a matter of fact, Watson has in the original text (New York, 1924, page 41) the words 'good' and 'bad' in quotation marks plainly as citations from everyday speech. Moreover, without prejudice to Watson's merits as an investigator and as a popularizer, his Behaviorism has the familiar faults of popularization and cannot be seriously used as Neurath uses it.

The realization that science can speak only in physical terms will not
down.   One cannot read modern writings without meeting it again and
again, expressed by students who, to all appearance, have reached it
independently.   The early papers of Pavlov show dramatically how a
group of physiologists is forced to accept this discipline.[7]   In England,
Lancelot Hogben demonstrates keenly and brilliantly how biology forces
it upon us.[8]   For physical science it is a working rule, but even when
physicists look beyond this, some of them arrive at our conclusion.
Thus, P. W. Bridgman, in spite of a perfunctory and otiose profession
of mentalism, and in spite of much animistic verbiage which could be
easily translated away—Bridgman always says 'concept' when he means
simply 'word' or 'technical term'—formulates and applies an 'opera-
tional' rule for all definitions in physics, to the effect that terms which
do not speak of operations are meaningless.[9]   Doubtless also there is
more than one isolated instance, such as the medical dissertation of H.
Ahlenstiel, Der Begriff psychisch und die Auffaussungen vom Wesen

In order to compare the Vienna students' physicalism with serious American
work, one must study the latter as well as the former.

Yet Neurath's point is not without interest.   In correcting the passage from
Watson, he can find only an ethnological translation for the words *good* and *bad*;
meanwhile Weiss, Theoretical Basis 102ff., 446ff. has given a strictly physical
translation of these words in terms of the *variability of a system*.

Neurath proposes (Einheitswissenschaft 17) to designate his group as *Behaviori-
stiker* and their discipline as *Behavioristik*, in contrast with the American *behavior-
ists* (*Behavioristen*) and *behaviorism* (*Behaviorismus*).   The distinction is illusory,
since it is based upon lapses from exactitude, real or apparent, such as are to be
found also in the writings of the Vienna circle.   We shall do well not to insist
upon such deviations, but rather to concentrate upon the necessary and sufficient
rule: Every scientific statement is made in physical terms.   The most perfect
formulation of this, so far as I know, and the best exemplification, are to be found
in Weiss's Theoretical Basis.   As to the name *behaviorism* (which Weiss disliked),
it is in many ways objectionable and has been adopted by writers who fail, not
only in the way of lapses, but in actual operation, to fulfil the essential demand
(Carnap, Erkenntnis 3.125, 'unechter Behaviorismus').   *Physicalism* is a much
better word.   We should stress our agreement as to the essential point and join in
defending it from misinterpretation.   Note, for instance, the striking accord
between Neurath's 'zweites Menschlein' (Einheitswissenschaft 16) and Weiss's
early essay, 'The mind and the man within', Psychological Review 26. 327 (1919).

[7] I. P. Pavlov, Conditioned Reflexes, translated by G. V. Anrep, Oxford 1927.

[8] L. Hogben, The Nature of Living Matter, London 1930.

[9] P. W. Bridgman, The Logic of Modern Physics, New York 1932; profession of
mentalism (x); operational principle (5); application (28, an excellent example,
whose very wording agrees with Carnap); 56 (an important point); 94; 130; 139;
153; 166; 203.

der Wissenschaft.[10] All these students, however, like their predecessors, the 'materialists' of the eighteenth and nineteenth centuries, are left with the problem: How do ideas arise from mere matter?[11] The students of the Vienna group, alone, it would seem, agree with their American colleagues in viewing this as a pseudo-problem, because such terms as 'idea' are merely misnomers for linguistic events.

*   *   *   *   *

The testing of this hypothesis of *physicalism* will be a task of the next generations, and linguists will have to perform an important part of the work. Non-linguists (unless they happen to be physicalists) constantly forget that a speaker is making noise, and credit him, instead, with the possession of impalpable 'ideas'. It remains for linguists to show, in detail, that the speaker has no 'ideas', and that the noise is sufficient—for the speaker's words act with a trigger-effect upon the nervous systems of his speech-fellows. Linguists, then, will have to read the description of the universe, as men have written it, and wherever they come upon the mention of an 'idea' (or any synonym, such as 'concept', 'notion', or the like), they will have to replace this mention by terms relating to language. If the description so revised is better than the old—simpler and fruitful of sounder and easier prediction—, then the hypothesis will have been confirmed and mankind will accept it as we accept the Copernican astronomy.

We may illustrate this by a simple instance of a typical sort. Here is a passage from a most admirable treatise on the foundations of scientific method:[12]

> The geometrical ideas of line and plane involve absolute sameness in all their elements and absolute continuity. Every element of a straight line can in conception be made to fit every other element, and this however it be turned about its terminal points. . . . Further, every element of a straight line or plane, however often divided up, is in conception, when magnified up, still an element of straight line or plane.
>
> The geometrical ideas correspond to absolute sameness and continuity, but do we experience anything like these in our perceptions? . . .

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[10] Printed summary on a leaflet, Kiel 1921; the original dissertation is typewritten only, and I have not seen it.

[11] Here we must include the doctrine predominant among Russian scholars. For example, R. Shor's article on Linguistics in the Encyclopedia (Bol'shaja sovetskaja enciklopedija, 65.392, Moscow 1931) represents not 'materialism' in any strict sense, but rather the normal nineteenth-century dualism.

[12] Karl Pearson, The Grammar of Science[3] 1.197 (London 1911).

The fact remains, that however great care we take in the preparation of a plane surface, either a microscope or other means can be found of sufficient power to show that it is not a plane surface. It is precisely the same with a straight line; however accurate it appears at first to be, exact methods of investigation invariably show it to be widely removed from the conceptual straight line of geometry. . . . *Our experience gives us no reason to suppose that with any amount of care we could obtain a perceptual straight line or plane, the elements of which would on indefinite magnification satisfy the condition of ultimate sameness involved in the geometrical definitions.* We are thus forced to conclude that the geometrical definitions are the results of processes which may be started, but the limits of which can never be reached in perception; they are pure conceptions having no correspondence with any possible perceptual experience.

The terms 'perceptual' and 'conceptual' derive from the following consideration: 'My universe consists necessarily and exclusively of my experiences'. This, the solipsistic axiom, tells us nothing about anything *within* the universe; whatever its value for other activities, it has no bearing on science. Hence for 'perceptual' we shall say *actual*, and for 'conceptual' we shall say *verbal*.

Pearson speaks here of three things: (1) actual ('perceptual') objects, (2) speech-forms, namely geometrical definitions, and (3) 'ideas' or 'concepts', such as 'the concept of a straight line'. It is our hypothesis that (3) is merely a traditional but useless and confusing way of talking about (2); that we find in our universe (that is: require in our discourse) only (1) actual objects and (2) speech-forms which serve as conventional responses to certain features that are common to a class of objects.

Suppose that we know nothing of geometry. We have a great many little spots all over the floor, including two red ones, some distance apart, which we will call A and B; and we have a great many rods or strips of metal, of various shapes. We take these rods of metal and lay first one then another so as to cover both of the red spots A and B. We soon find that the metal rods are of two kinds. Some of them, when we lay them so as to cover A and B, cover always the same other spots, no matter how we lay them or which rod we use. We call these rods 'straight'. Of the remaining rods this is not true; one and the same rod can be laid in various ways so as to cover spots A and B; it will cover now some of the black spots and now others. Any two rods of this second class can be laid so as to cover different spots, always including A and B.

The geometrician gives us a succinct statement of this. Given a set of things called 'points', we define classes of these. The classes called 'straight lines' are classes such that on any two points there is one and only one straight line.

Now, it is probable that if we make the spots very small and place them very close together and examine the placing of the rods with a microscope, no rod will ever satisfy the geometric definition of 'straight'. Also, this definition does not mention the width, thickness, weight, temperature, color, and so on, of the rods, although every rod presents features of this sort. In these respects the term 'straight' resembles all other speech-forms; it is in such forms that we discourse and cooperate.

Now let us re-word the statement in non-mentalistic terms:

> The geometrical definitions of line and plane say that lines and planes are absolutely alike in all their elements and absolutely continuous. They say that every element of a straight line can be made to fit every other element, and this however it be turned about its terminal points. . . . Further, every element of a straight line or plane, however often divided up, is still, according to the geometrical definition, an element of straight line or plane.
>
> The geometrical definitions imply absolute sameness and continuity, but we find no objects with these characteristics. . . .
>
> The fact remains, that however great care we take in the preparation of a plane surface, either a microscope or other means can be found of sufficient power to show that it is not a plane surface. It is precisely the same with a straight line; however accurate a straight edge appears at first to be, exact methods of investigation invariably show it to be far from satisfying the geometric definition. . . . Our measurements give us no reason to suppose that with any amount of care we shall ever obtain a straight-edged or plane-surfaced object which will under careful observation satisfy the geometric definitions. We are thus forced to conclude that the geometrical definitions are simple verbal descriptions which roughly describe classes of objects but do not exactly describe any given object. In this the geometrical terms are like all other speech-forms.

It is our hypothesis that the terms 'concept', 'idea', and so on add nothing to this. We suppose that the person who says 'I was having an idea of a straight line' is telling us: 'I uttered out loud or produced by inner speech movements the words *straight line*, and at the same time I made some obscure visceral reactions with which I habitually accompany the sight or feel of a straight edge or the utterance or hearing of the word *straight*.' Of all this, only the verbal action is constant from person to person. If we are right, then the term 'idea' is simply a traditional obscure synonym for 'speech-form', and it will appear that what we now call 'mental' events are in part private and unimportant events of physiology and in part social events (responses which in their turn act as stimuli upon other persons or upon the responder himself), namely acts of speech. If this is true, then linguistics in the future will deal with much wider problems than today.

LINGUISTIC THEORY IN THE ITALIAN RENAISSANCE

Robert A. Hall, Jr.

[The work of Renaissance scholars on language, especially in Italy, deserves more attention than it has usually received. Certain sixteenth-century writers (Tolomei, Castelvetro, Scaliger, and others) anticipated nineteenth-century developments: firstly, in regarding language as a social phenomenon; secondly, in recognizing change as an essential element of linguistic history; and thirdly, in a more scientific approach, particularly in regard to phonetic law.]

Historians of linguistics usually set the date of the beginning of scientific linguistic study in Europe towards the end of the eighteenth or the beginning of the nineteenth century, and consider the Europeans' discovery of Sanskrit and the Hindu grammarians as a principal cause of this advance.[1] The work of the European scholars of the Renaissance tends to receive comparatively slight attention, although these scholars displayed a great interest in linguistic questions, and published many writings on the subject of language. This neglect is unquestionably due to the unfavorable impression which these writings make on first acquaintance—for they contain a very small proportion of valuable ideas and material, beside a great amount of fanciful and ill-founded notions. The scholars of the Renaissance had (as it seems from four centuries later) very little material at their disposal, and their work is replete with irrelevancies. As a result, the Renaissance scholars' work is often regarded as of interest merely as a curiosity, if not, indeed, as quite worthless.[2]

Yet on closer examination some of these works prove to contain valuable material—especially in Italy, where debates on language were most intense and the matter of the standard language was most thoroughly discussed. Indeed, it becomes clear that the points of general theory

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[1] Cf. Bloomfield, Language 9, 11ff. (New York, 1933).

[2] E.g. Labande-Jeanroy, La question de la langue en Italie 5 (Strassburg, 1925); 'La question de la langue ne mérite en aucune manière de retenir l'attention des érudits, des critiques littéraires, ou des linguistes; que les controverses qu'elle suscita ne furent que des querelles de pédants. . . .'

and philosophy of language and linguistic change which were developed in the course of the Renaissance debates, mark a considerable advance over the notions which had prevailed in previous centuries, and are worthy of far more serious consideration than has hitherto been accorded them.   A number of Renaissance concepts—of the connection between language and society, of the change of languages in the course of time, and of a more scientific approach—were developed 'at home' with intelligent and fruitful use of the available data, and anticipate by three centuries developments ordinarily ascribed to the nineteenth century.

It is proposed here, therefore, to outline briefly the advances and improvements in linguistic theory which were made in the course of the Italian Renaissance debates on the questione della lingua.   We shall consider first the Cinquecento theorists' ideas on the relation of language to the individual and society, on language as an independent 'organism' and as a norm to which conformity might be demanded; secondly, their conception of linguistic change and of the origin of the Romance tongues; and finally, the improvement in scientific approach brought about chiefly by Tolomei, Castelvetro, and Scaliger.[3]

These disputes originated and were carried on mainly with reference to the written language and to its use in literature; but rather more attention was given than has sometimes been supposed, to questions of the spoken language and of speech as a human institution.   At least one work (Varchi's 'Ercolano') was written with the definitely stated purpose of considering only spoken language, and several other men's work (Tolomei in the 'Cesano' and the 'Polito', and Castelvetro in part of the 'Giunte' and the 'Correzioni') was concerned chiefly with spoken usage.   Not all the writers of the sixteenth century saw the necessity of distinguishing between the spoken and the written word, between the sound and the letter; but that this prime necessity of distinction was well recognized in some quarters, and for the first time, is obvious from the heated controversies in regard to orthographic reform,[4] and from

[3] For the detailed history of the linguistic discussions in sixteenth-century Italy, see chiefly Vivaldi, Storia delle controversie intorno alla questione della lingua (Catanzaro, 1925); also Belardinelli's La Questione della Lingua (Rome, 1904) and Trabalza's Storia della Grammatica Italiana (Milan, 1908).

[4] See Zambaldi, Delle teorie ortografiche in Italia, in Atti del R. Istituto Veneto di Scienze, Lettere ed Arti (Serie VII, vol. III); and F. Sensi, M. Claudio Tolomei e le controversie sull'ortografia italiana nel secolo XVI, in Atti dell'Accademia dei Lincei (Serie IV, vol. VI).

statements by Julius Caesar Scaliger ('Ab sonu est iudicandum, non ab literâ'[5]) and Benedetto Varchi:

> Lo scrivere non è della sostanza delle lingue, ma cosa accidentale, perchè la propria, e vera natura delle lingue è che si favellino, e non che si scrivano, e qualunque lingua si favellasse, ancorachè non si scrivesse, sarebbe lingua a ogni modo, e se fosse altramente le lingue inarticolate non sarebbono lingue, come elle sono.[6]

Investigations into phonetics and the nature of sound were carried out by several scholars, including Bartoli,[7] J. C. Scaliger (who had an especial interest in the physiology of sound-production because of his profession as doctor), and Tolomei in the 'Polito'. But the nature of language itself was not summarized very far beyond a general formula such as that of Varchi, 'Il parlare, ovvero favellare umano, non è altro che manifestare ad alcuno i concetti dell' animo mediante le parole'.[8] In regard to the 'natural' or 'artificial' origin of words, the Scholastics' synthesis of the old so-called 'Platonic' and 'Aristotelian' viewpoints,[9] which had been expressed by Dante in the well-known passage of the 'Paradiso',[10] continued to be accepted throughout the Cinquecento in Varchi's and others' support of the *ad placitum* doctrine, with only a few unimportant exceptions. The same is true of the traditional view of the connection between language and thought, which was in the Cinquecento, even as in ancient and mediaeval times,[11] conceived of as being quite loose, and language and thought as being easily separable and distinct.[12]

[5] De Causis Linguae Latinae 18 (Lyon, 1540).

[6] Ercolano 203 (Quesito Secondo) (ed. Padua 1764, together with Machiavelli's Dialogo, Castelvetro's Correzione and Muzio's Varchina, page references to all of which will be to this edition). In this passage, *lingue inarticolate* means 'quelle lingue, le quali scrivere non si possono, come ne sono molte tra le nazioni barbare. . . . .' 207 (Quesito Terzo).

[7] See E. Teza, Un Maestro di Fonetica Italiana nel '500, in Studî di Filologia Romanza 6.449–463.

[8] Ercolano 92 (Dubitazione Prima).

[9] See Rotta, La Filosofia del Linguaggio nella Patristica e nella Scolastica 247 f. (Turin, 1909).

[10] Paradiso XXVI 130–132.

[11] E.g., St. Anselm, Monoloquium 10; and Dante, De vulgari Eloquentiâ I. iii. 2.

[12] Cf. Scaliger, De Causis 114f.; the constantly recurring similes in Bembo, Varchi, Tolomei, etc., comparing language to a 'clothing' for thought and the like; or the phrase Tolomei uses in the Cesano, 'sciogliendo quel nodo a' pensieri che li sosteneva, così nella lingua scender gli fece', Cesano 16 (ed. Milan 1864, together with Trissino's Castellano).

But in regard to language in its relation to the individual and society, the Renaissance scholars' outlook differed radically from anything that had gone before.   The general tendency to regard the Cinquecento as an age of pure individualism has led[13] to the idea that language, also, was regarded in the Renaissance (as it had been in Scholastic philosophy[14]) solely from the point of view of the individual.   As a matter of fact, if Rotta's presentation of Scholastic thought on this point is correct, the Cinquecento must be considered as the time in which language first came to be regarded as a primarily social phenomenon.   This, too, in spite of the fact that almost all the writers on language recognized more fully than ever before the existence of differences in speech, not only from one speech-group to another, but from one individual to another. The best-known passage in regard to individual differences is that in the 'Castellano' of Trissino:

... perocchè ciascun uomo, e casa, e contrada, ha qualche particulare proprietà di parlare, che l'altro non ha; verbigrazia, Palla mio fratello ha qualche particulare proprietà del suo parlare, che non l'ho io; e Lorenzo vostro fratello n'ha qualcuna che non l'avete voi, e così parimente la casa nostra ha qualche differenza di parlare dalla vostra, e la nostra contrada da un' altra, e simili.[15]

This recognition of the individual as the prime linguistic unity was a general Renaissance concept, and not merely limited to the 'anti-Tuscan' group, as has been asserted;[16] for in 'Tuscan' writers as well, such as Tolomei in his 'Cesano' and Varchi in the 'Ercolano', we find frequent similar statements.   The latter at one point even uses this doctrine to combat Trissino's use of the term 'Italian pronunciation':

E ardirei di dire che non pure tutte le città hanno diversa pronuncia l'una dall'altra, ma ancora tutte le castella; anzi chi volesse sottilmente considerare, come tutti gli uomini hanno nello scrivere differente mano l'uno dall'altro, così hanno ancora differente pronunzia nel favellare; onde non so come si possa salvare il Trissino, quando dice nel principio della sua Epistola a Papa Clemente; *Considerando io la pronunzia Italiana.*[17]

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[13] Cf. the attitude of Belardinelli, op. cit., who, while approving the viewpoint of Muzio and the anti-Tuscans because of the broader 'national' viewpoint implied in their philosophy, laments the non-attainment of this ideal because of the general individualistic tendency of the period, and especially the devotion shown to their local dialect by the Tuscans, which he considers 'individualism' as representing the point of view of a small group as opposed to a national one.

[14] See Rotta, op. cit. 246.

[15] Trissino, Castellano 32 (ed. Milan 1864, vide supra).

[16] Labande-Jeanroy, op. cit. 18.

[17] Ercolano 203 (Quesito Primo).

But neither Trissino, an 'anti-Tuscan', nor Varchi, a 'Tuscan', was willing to grant any importance to this recognition of the individual differences of speech, since both of them wished to take into effective account only dialectal differences.   Trissino justified himself in this with a philosophical consideration, that of the impossibility of descending below the 'specie specialissima';[18] and Varchi protests in one place against what is essentially his own recognition of individual differences:

> . . . ben è vero, che la diversità e la differenza non è nè tanta, nè tale, che non si possano, chi sottilissimamente guardare non la vuole, sotto la lingua Fiorentina comprendere, perchè altrimente bisognerebbe non dividere le lingue, ma minuzzarle; non farne parti, ma pezzi; e brevemente, non distinguerle, ma stritolarle e farne minuzzoli.[19]

At another point he goes farther still, and declares that to be a true language, a linguistic system must be used not only by any given group of persons, but by an entire people, otherwise it is but a jargon and not a language:

> *Varchi*.   Lingua, ovvero linguaggio, non è altro che un favellare d'uno o più popoli, il quale o i quali usano, nello sprimere i loro concetti, i medesimi vocaboli nelle medesime significazioni e co' medesimi accidenti.
> *Conte Ercolani*.   Perchè dite voi *d'un popolo?*
> *Varchi*.   Perchè, se parecchî amici, o una compagnia, quantunque grande, ordinassero un modo di favellare tra loro, il quale non fosse inteso, nè usato se non da sè medesimi, questo non si chiamerebbe lingua, ma gergo, o in alcuno altro modo, come le cifere non sono propriamente scritture, ma scritture in cifera.[20]

Here (if we discount the slight confusion between an argot and a separate private language) we have language considered as indissolubly related to an imaginary social and political organism.   Tolomei and others also considered the prime purpose of language to be that of social intercourse.

Not only theories of the 'social organism', but also theories of language itself as an independent organism were present in some Cinquecento thinking.   Varchi makes a comparison of the various stages of a language with the four 'ages' of man;[21] nor was he the only Renaissance writer to make such a comparison, since G. B. Gelli draws a detailed

[18] Castellano 32.
[19] Ercolano 234 (Quesito Quinto).
[20] Ercolano 200 (Quesito Primo).
[21] Ercolano 225 ff. (Quesito Quinto).

parallel between the development of a language and the condition of a
human body in its different stages, and concludes:

> . . . Adunque egli è necessario in tutte le cose che dopo il principio loro hanno
> accrescimento e dicrescimento di perfezione, che e' si ritruovi tra l'uno e l'altro
> un certo spazio di tempo, nel quale elle restino di acquistarne più, e non comincino
> ancora a perdere; il quale tempo è chiamato da' filosofi lo stato. . . .[22]

Although it was mostly 'Tuscans' who held such theories, the idea was
widespread enough for Girolamo Ruscelli, an 'anti-Tuscan', also, to
make a comparison of language with a plant, and to draw a parallel
with the grafting of pear and plum trees.[23]

Others, of course, objected to this unjustified use of metaphor, and
Peretto Pomponazzi, in Speroni's Dialogo delle Lingue', basing his
arguments on the 'Aristotelian' theory of *beneplacitum*, declares:

> . . . io non vorrei che voi ne parlaste come di cosa dalla natura prodotta,
> essendo fatte < le lingue>, e regolate dallo artificio delle persone a beneplacito
> loro, non piantate, nè seminate. . . . Dunque non nascono le lingue per sè mede-
> sime, a guisa d'alberi, o d'erbe, quale debbole et inferma nella sua spezie, quale
> robusta et atta meglio a portar la somma di nostri umani concetti; ma ogni loro
> vertù nasce al mondo dal voler de' mortali.[24]

Trissino as well, in the person of the Castellano Rucellai, says sarcas-
tically to Filippo Strozzi

> . . . lasciando stare il fatto che voi fate le lingue aver principio, vita e fine,
> come le febbri. . . .[25]

The idea of correctness was, of course, predominant in the Renais-
sance as it is today, both as a heritage from ancient and mediaeval
times, and in connection with aesthetic ideas of regularity and the imita-
tion of models;[26] and also in connection with intellectual or social stand-
ing.   This idea was common to most 'Tuscans' and 'anti-Tuscans'

[22] Gelli, Opere 309 f. (ed. Florence, 1855). This conception of 'accrescimento,
stato e dicrescimento' is, of course, exactly the same as that applied by Humboldt
and Bopp to the 'growth' (pre-IE period), 'state of perfection' (IE period) and
'decay' (post-IE period) of the Indo-European tongues. (Cf. Humboldt, Vers-
chiedenheit §19 and Bopp Vokalismus 1, 2.)

[23] Ruscelli, Tre Discorsi a M. Lodovico Dolce: Lettera Seconda 67 (Venice,
1553).

[24] Speroni, Dialogo delle Lingue 72, 73 (ed. Lanciano, 1912).

[25] Castellano 34.

[26] Especially in the case of Bembo; see Trabalza, Storia della Grammatica
Italiana 65, etc.

alike.   But the stand taken by Peretto Pomponazzi, an interlocutor in
Speroni's 'Dialogo', is important in exemplifying a reaction against
humanistic ideas of correctness or of a necessary limitation of thought
by linguistic form.   Peretto vigorously asserts the equal value of all
languages, even the Arabic and the Hindu (though it is not to be thought
from this reference that he had direct knowledge of them) for philo-
sophic discussion:

> Io ho per fermo, che le lingue d'ogni paese, così l'Arabica e l'Indiana, come la
> Romana e l'Ateniese, siano d'un medesimo valore, et da mortali ad un fine con un
> giudizio formate . . . le quali usiamo sì come testimoni del nostro animo, sig-
> nificando tra noi i concetti dell'intelletto.[27]

To the humanist Lascari's contention that 'diverse lingue sono atte a
significare diversi concetti, alcune i concetti di dotti, alcune altre degli
indotti', he replies:

> Più tosto vo' credere ad Aristotile, et alla verità, che lingua alcuna del mondo
> (sia qual si voglia) non possa aver da sè stessa privilegio di significare i concetti
> del nostro animo; ma tutto consista nello arbitrio delle persone, onde chi vorrà
> parlar di filosofia con parole Mantovane o Milanesi, non gli può esser disdetto a
> ragione, più che disdetto gli sia il filosofare, et l'intender la cagion delle cose.[28]

This pro-dialectal thesis of Peretto in Speroni's work is essentially
'anti-Tuscan' in its relation to the questione della lingua; but a similar
philosophy in regard to adherence to established rules is expounded by
Gelli, a 'Tuscan', in his 'Capricci del Bottaio', where Giusto the cobbler's
'Anima' assures him that:

> tutte le lingue . . . sono atte ad esprimere i concetti e i bisogni di coloro che le
> parlano; e quando pure elle fussino altrimenti, quei che l'usano le fanno.[29]

The difference between Latin and Italian and at the same time the
obvious relationship between them, inevitably suggested to the minds of
the Renaissance writers the question of linguistic change, and why
and how the Italian language had developed out of Latin.   Dante was

[27] Dialogo 72.
[28] Dialogo 74.   It is perhaps worth noting that, although Speroni apparently
favors Peretto's viewpoint in regard to language as an instrument of philosophic
or scientific discussion, he seems on the other hand to give Bembo and his pro-
Tuscan, archaistic doctrine the last word in so far as the literary use of language
is concerned; Dialogo 83, 84.
[29] Gelli, Opere 213.

the first writer on language to evolve a theory of inevitable linguistic mutability.[30]   In the De vulgari Eloquentiâ, he had written:

Cum igitur omnis nostra loquela, praeter illam homini primo concreatam a Deo, sit a nostro beneplacito reparata post confusionem illam, quae nil fuit aliud quam prioris oblivio, et homo sit instabilissimum atque variabilissimum animal, nec durabilis nec continua esse potest; sed sicut alia quae nostra sunt, puta mores et habitus, per locorum temporumque distantias variari oportet.[31]

He had later declared, (abandoning, however, both the notion of the special creation of a language for Adam by God and the idea of its duration up to the time of the tower of Babel) in the famous passage of the Paradiso:

La lingua ch'io parlai fu tutta spenta
  innanzi che all'ovra inconsummabile
  fosse la gente di Nembròt attenta;
Chè nullo effetto mai razionabile
  per lo piacere uman che rinovella
  seguendo il cielo, sempre fu durabile.
Opera naturale è ch'uom favella,
  ma così o così, natura lascia
  poi fare a voi, secondo che v'abbella.[32]

The fact of linguistic change was, by the time of the Cinquecento, accepted without discussion, and debates centered rather around its nature and causes.   One of the first questions to arise was that of the origin of Italian.   Dante, without indicating his belief as to the actual order of development, had held that Latin had been established by a consensus of opinion to serve (as it did in his time) as a common language over and above the vulgar tongues.[33]   This doctrine bears some similarity to that of Leonardo Bruni, of the existence of modern Italian, in its present form, at the time of the Romans as the language of the

[30] Horace, Ars Poetica 60–62, 69–72, and similar passages do not seem to me to constitute an exception to this statement, since they refer only to the vocabulary and not to the phonetic and morphological aspects of language as well.

[31] De vulgari Eloquentiâ I. ix. 6 (the text here, as in all other quotations from Dante, is cited from the Testo Critico della Soc. Dantesca Italiana, Florence, 1932).

[32] Paradiso XXVI 124–132.

[33] De vulgari Eloquentiâ I. ix. 11.   Whether Dante thought the languages of oc, oïl and sì to have been developed out of the grammatica or vice versa depends on whether one interprets literally or figuratively the word inniti in the phrase by which he characterizes Italian: Magis videtur inniti grammaticae, quae comunis est (De vulgari Eloquentiâ I. x. 4).

lowest classes; of which we find a modified echo in Castelvetro's 'Giunte', where he maintains that modern Italian in its essentials (regarding accidence as comparatively unimportant) was the language of the lower classes of ancient Rome.[34]

But in general it was considered that the vulgar tongue had resulted from a mixture of Latin with the language of the barbarian invaders,[35] to a greater or less degree, and heated arguments occasionally arose over the exact details of what tribes in what places had originated the new language.[36]   Not only the tongues of the Germanic invaders, but also Hebrew,[37] Provençal,[38] and Etruscan[39] were cited as possible sources for morphological, as well as lexicological, innovations in Italian.   An interesting passage is that in which Muzio, piqued by the accusations of impurity leveled against Italian by the humanists, turns their own weapons against them, and advances a theory of the origin of Latin from the combination of Etruscan with the language of the invaders:

> Tale istimo io adunque, che fosse l'origine di quella lingua, che con le prime lingue, quelle delle sopravegnenti nationi corrompendosi, ne fosse la nuova latina generata. . . . e questo è intorno a ciò la mia ferma opinione, la qual se vera è, non so come veramente dicono, biasimando la nostra, per esser ella provenuta del mescolamento di più lingue, che quella sia così semplice e pura.[40]

In general, linguistic change was presumed to occur usually and in any case much more rapidly in the case of invasion and settlement of territory by new peoples, than as a result of gradual evolution of a language's 'inner form' of its own accord.   This viewpoint we find elaborated in Tolomei[41] and in Machiavelli:

[34] Castelvetro, Correttione . . . et una Giunta al primo libro delle Prose di M. Pietro Bembo (with each section of the Prose printed before the corresponding section of the Giunta; references to the Prose will be made by page to this edition) 136, 147 (Basel, 1572).

[35] Tolomei, Cesano 54, 55; Ruscelli, Lettera Seconda al Dolce 59; Castelvetro, Giunta 153, 154; Muzio, Battaglie, Lettera al Trivulzio f.9b (ed. Venice, 1582); Bembo, Prose 149, 150; etc.

[36] E.g. the polemic between Varchi and Muzio; the interesting point is the common ground of belief in the precisability in minute detail of the time and place of these occurrences.

[37] Tolomei, Cesano 69.

[38] Varchi, Ercolano 264, 271.

[39] Muzio, Battaglie, Lettera al Trivulzio f.9b.

[40] Muzio, Battaglie, Diffesa della volgar Lingua f.157b.

[41] Cesano 52.

Ma è ben vero che col tempo, per la moltitudine di nuovi vocaboli, imbastardiscono le lingue e diventano un'altra cosa; ma fanno questo in centinaia d'anni; di che altri non s'accorge se non poi che è rovinata in una estrema barbarie. Fa ben più presto questa mutazione, quando egli avviene che una nuova popolazione venisse ad abitare in una provincia. In questo caso ella fa la sua mutazione in un corso d'un'età d'un uomo.[42]

A great part of Renaissance linguistics was conducted on the same lines as ancient etymologizing, with very little conception of rigorous method; and, as in ancient etymologies of the 'lucus a non lucendo' type, more attention was paid to the explanation of a word's origin by comparison with words of similar appearance, even if of opposite meaning, than to investigation of the development of its phonetic elements. Varchi accepted this as the normal state of affairs, hence his well-known contempt for the fanciful and often ridiculous imaginings of the etymologists. This carried him too far, however, in the direction of an exaggeration of the ad placitum theory in regard to phonetics as well, and of a denial of any possibility of explaining the development of words. The passage at the very end of the 'Correzione', where Castelvetro narrates his discussion with Varchi on the origin of the Italian future, is very well known; in which, replying to Varchi's contempt for the idea of regular phonetic development, Castelvetro sets forth and proves from the examples, not only of *amerò*, but also of *leggerò* and *udirò*, the rule of the preservation intact of -*r*- from Latin to Italian, and of the origin of the composite future.

However, Castelvetro's more or less hazy intuition of regular sound-change is not of so much importance as a foreshadowing of modern linguistic science, as the work of Claudio Tolomei, who may properly be hailed as the real forerunner of orderly, scientific examination of linguistic change. Treating of the development of *plenus, clavis,* and [*af*]*flatus* into *pieno, chiave,* and *fiato,* he says:

   . . . e ardirei dire che nel primo e puro parlar degli uomini toscani questa fosse universale e verissima regola, e tutti quei vocaboli, che ora altrimenti s'usano e scritti si trovano, come *plora, implora, splende, plebe* e simili, non fussero presi dal mezzo delle piazze di Toscana; ma poste innanzi dagli scrittori, e da qualche ingegno, che volse la lingua arricchire, che gli parse usargli, come nelle stampe latine gli trovò, senza dar loro forma di toscan parlare . . . perchè senza dubbio il comune uso di quel secolo averebbe, se egli avesse quei vocaboli ricevuto, *piora, impiora, spiende* e *pieve* detto, come di questo ultimo ne abbiamo manifesto segno,

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[42] Dialogo intorno alla Lingua 522.

che volgarmente Pieve si chiama quella sorte di chiesa ordinata alla religione di una plebe.[43]

Here, Tolomei anticipates fully the chief contributions of nineteenth-century linguistic science. The hypothesis of regular sound-change; the distinction, which immediately follows therefrom, between 'popular' and 'learned' words, and the recognition of pairs of 'doublets' with different meanings, representing different stages of the language; and the study of a word first from the phonetic point of view and then from the semantic, are all present in this passage from Tolomei, not only in embryo but fully developed. All that would have been necessary for the development of linguistic science two hundred and fifty years previous to its actual rise, would have been the careful application of Tolomei's method; unfortunately, outside of Celso Cittadini (who took over Tolomei's ideas,[44] but did not handle them exceptionally well), Tolomei had no successor, and hence one cannot call him the 'father', but only a very advanced precursor of modern linguistics.

In addition to Castelvetro and Tolomei, J. C. Scaliger deserves mention as another in advance of his time with respect to scientific procedure; though evidences of this are more fragmentary than in the case of the other writers. Especially important are his attempts at physiological explanations of sound-developments and other phenomena, and his use of what few data were available on ancient Latin forms to outline a few traits of historical grammar, as *NATUS, fuit enim GNATUS, a GENEROR*.[45] Unfortunately, his information was often scanty or his intuitions as to the historical order of his data were at fault, so that his contribution remained quantitatively small, despite the improvement in method. The three above mentioned, however, mark a definite advance over the haphazard approach characteristic of the grammarians of the preceding epochs and of that century—an approach defended by Varchi as a necessary evil.

In spite of the continued prevalence of the traditional outlook on grammar among most of the grammarians and lesser scholars of the century, therefore, a considerable advance was made in linguistic sci-

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[43] Cesano 66, 67.

[44] See F. Sensi, Per la Storia della Filologia Neo-latina in Italia—Claudio Tolomei e Celso Cittadini, in Arch. Glott. It. 12.441–60. Cittadini was for some time thought to have been the first to initiate scientific treatment of historical language study; but Sensi's article proves completely that this honor belongs to Tolomei, from whom Cittadini derived his method.

[45] De Causis 37.

ence during the sixteenth century in three respects: in regarding language as a social phenomenon; in recognizing, from the time of Dante on, the necessary and inevitable change continually taking place in language; and in beginning a more careful and rigorous method of investigation. Hence the date of the 'dawn' of modern linguistic science must be displaced from the nineteenth to the sixteenth century, with the work of Castelvetro, Scaliger, and Tolomei, the last of whom deserves full recognition as the anticipator of modern method.

SOME HITTITE WORDS IN *ta*-

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[Discussion of *ta, takku, tamais, tan, tayukas, takiya*, also (in connection with *takiya*) *nakkus*.]

1. *ta*

The relationship between the Hittite sentence-connective *ta* and the IE pronominal stem **to-* is discussed in HG 199–200,[1] where it is assumed

[1] Abbreviated forms used in bibliographical citations are to be interpreted as follows: AU = Ferdinand Sommer, Die Aḫḫijavā-Urkunden (Munich, 1932) = Abhandlungen der Bayerischen Akademie der Wissenschaften Philosophisch-historische Abteilung, N. F. 6; Br. = K. Brugmann, Abrégé de grammaire comparée des langues indo-européennes, translated into French by J. Bloch, A. Cuny, and A. Ernout (Paris, 1905); Chr. = Edgar H. Sturtevant and George Bechtel, A Hittite Chrestomathy (Philadelphia, 1935); Denkschr. = Denkschriften der Kaiserlichen Akademie der Wissenschaften [of Vienna], Philosophisch-historische Classe; EM = A. Ernout and A. Meillet, Dictionnaire étymologique de la langue latine (Paris, 1932); Gl. = Edgar H. Sturtevant, A Hittite Glossary[2] (Philadelphia, 1936); Gr. = Karl Brugmann and Berthold Delbrück, Grundriss der Vergleichenden Grammatik der Indogermanischen Sprachen[2] (5 vol., Strassburg, 1893–1916); HG = Edgar H. Sturtevant, A Comparative Grammar of the Hittite Language (Philadelphia, 1933); Hat. = Albrecht Götze, Ḫattušiliš, der Bericht über seine Thronbesteigung nebst den Paralleltexten (Leipzig, 1925) = Mitteilungen der Vorderasiatisch-Aegyptischen Gesellschaft 29.3; Hrozný = Frédéric Hrozný, Code hittite provenant de l'Asie Mineure (Paris, 1922); IF = Indogermanische Forschungen; Jahrb. für Cl. Ph. = Jahrbücher für Classische Philologie; KZ = Zeitschrift für Vergleichende Sprachforschung; KlF = Kleinasiatische Forschungen; Lang. = Language; MS = Albrecht Götze and Holger Pedersen, Muršilis Sprachlähmung (Copenhagen, 1934); MV = A. Meillet and J. Vendryes, Traité de grammaire comparée des langues classiques (Paris, 1927); New Eng. Dict. = New English Dictionary, edited by James A. H. Murray (10 vol. and supplement, Oxford, 1888–1933); OLZ = Orientalistische Literaturzeitung; PBSB = Beiträge zur Geschichte der Deutschen Sprache und Literatur; SS = Stolz-Schmalz Lateinische Grammatik[5], revised by Manu Leumann and Joh. Bapt. Hofmann (Munich, 1928); Sommer, Hdb. = Ferdinand Sommer, Handbuch der Lateinischen Laut- und Formenlehre[2] (Heidelberg, 1914); Sommer, Krit. = id., Kritische Erläuterungen zur Lateinischen Laut- und Formenlehre (Heidelberg, 1914); TAPA = Transactions of the American Philological Association; WP = Alois Walde, Vergleichendes Wörterbuch der Indogermanischen Sprachen, edited and revised by Julius

that the former antedates the latter. However, I believe the existence of the demonstrative stem in Hittite may be posited, even though the demonstrative PRONOUN was not yet created; and it seems to me that we may recognize this stem as the basis of *ta*. For the derivation of a conjunction from the bare pronominal stem, cf. Gr. δέ from the demonstrative stem **do-/*de-*[2], Gr. -τε and Lat. -*que* from the indefinite stem **kwo- / *kwe-*.

The sense of *ta* would indicate connection with the demonstrative stem, for it regularly means 'then', not simply 'and', as is made clear by a study of its use in the Code[3]. It appears to indicate simultaneous action in §10 *ták-ku* LÚ.GÀL.LU-*an ku-iš-ki ḫu-u-ni-ik-zi ta-an iš-tar-ni-ik-zi*, translated by Sturtevant (Chr. 213) 'if anyone has a man bewitched and he makes him ill'; possibly also in §53 *ma-a-ni-za i-da-a-la-u-e-eš-ša-an-zi ta-az* É-ŠU-NU *šar-ra-an-zi*, 'if they become bad friends and divide their household'; but elsewhere in the Code it invariably indicates subsequent action, as in §43 *nu* KUN GUD *e-ip-zi ta* ÍD-*an za-a-i*, 'and he takes hold of the ox's tail and (then) crosses the river'.[4] It is thus to be compared with Skt. *tadá*, Lat. *tum, tunc, tandem*, Germ. *dann*, Eng. *then*, also from the demonstrative stem **to-*[5]. Similarly from the demonstrative stem **do-/*de-* are derived Skt. *tadá* (second part), Gr. δέ (cited above), δή, ἤδη, Lat. *dum, donec, deinde, denique*, etc.[6]

2. *takku*

Takku 'if' is regularly assumed to be a derivative of *ta*, which it resembles in two particulars, its initial position and its employment in

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Pokorny (3 vol., Berlin and Leipzig, 1927–1932); Walde = Alois Walde, Lateinisches Etymologisches Wörterbuch[3], revised by J. B. Hofmann (Heidelberg, 1930–); Walther = Arnold Walther, in J. M. Powis Smith, The Origin and History of Hebrew Law 246 ff. (Chicago, 1931); Whitney = William Dwight Whitney, Sanskrit Grammar[2] (Cambridge, 1923); ZA = Zeitschrift für Assyriologie; Zim.-Fried. = Hethitische Gesetze, translated by Heinrich Zimmern and Johannes Friedrich (Leipzig, 1922) = Der Alte Orient 23.2.

[2] Br. 655; Persson IF 2.218.

[3] The other common sentence-connective, *nu*, which later replaced *ta* altogether, also seems to have temporal force; its stem, **nu*, means 'now' (Br. 652) and it is to be associated with Skt. *nu* and Gr. νυ 'then' (HG 96), also with Gr. νῦν and Lat. *nunc* 'now' (EM 652).

[4] Cf. §§ 28, 46, 60, 61, 62, 86, 103 (bis), 107, 127, 146, 162, 163, 164, 165 (line 31), 166 (once, perhaps twice), 167 (bis), 168 (bis), 169 (bis), 171 (four times), 172, 193 (three times), 196, 198 (bis).

[5] WP 1.742–743.

[6] WP 1.769–771; Walde 335.

the early language. But its acquisition of the meaning 'if' is by no means self-evident. Temporal conjunctions easily become conditional —e.g., Skt. *yád* and *yádi*, Germ. *wenn*, not to mention *takku*'s own rival and supplanter *man*[7]—, but the first step in the process is the development from a temporal adverb or coördinating conjunction, such as *ta* is, into a subordinating conjunction ('when'), and anything of the sort seems utterly non-existent in the case of *ta*. A possible explanation might be that *ta* . . . *ta* were originally used correlatively; this would be quite in line with the IE use of *to- . . . *to-[8] as a variant of *yo-. . . *to- or of *kwo- . . . *to-. With such a pair as a starting-point, we frequently find that the first member acquires subordinating force, and the second is or may be entirely suppressed. Thus in Lat. the correlative use of *dum* . . . *dum*[9] and *simul* . . . *simul*[10] may well be assumed to have given rise respectively to the conjunctions *dum*[11] and *simul* or *simul ac*.

This explanation for the rise of *takku* has the additional advantage of accounting for the second element, which is presumably the particle -*ku* or -*aku*[12], since, if Sturtevant is right in deriving -*ku* from the indefinite-relative stem[13], it forms a perfect parallel etymologically for the Lat. -*que*[14], and thus semantically for *ac* of *simul ac*[15].

[7] On this cf. Götze, OLZ 1925.238.

[8] MV 570, 575.

[9] Seen in Plautus Truc. 232 and in Catullus 62.45. The reading has been questioned in both passages; but for the first it is well defended by Fleckeisen (Jahrb. für Cl. Ph. 1870.648), and for the second it is proved by Quintilian (9.3.16).

[10] Seen, e.g., in Vergil Aen. 1.631–2; 2.220–2.

[11] Cf. Hofmann (SS 742), even though he prefers a different explanation.

[12] See Götze, ZA NF 2.268; Hrozný, ib. 4.175.

[13] HG 120. The derivation seems to me highly plausible, although it is open to objection, as Professor Sturtevant himself has remarked to me, on the ground that it fails to provide an explanation for the *a* of -*aku*. But the relation of -*ku* and -*aku* can be thoroughly understood only when that of -*pa* and -*apa* is (on the stem *pu, *apu in IE, see WP 1.47–9; Walde 2; EM 3).

[14] Thus *kwiski*, by assimilation for *kwisku* (which form actually occurs—HG 120, 210), is in origin to be compared with Lat. *quisque*, though in use it corresponds rather to *quis* (in conditions) and to *quisquam* (in negations), the semantic parallel for *quisque* being *kwissa*. Also *ni-ik-ku*, presumably to be interpreted *nekku*, is perhaps to be compared with Lat. *neque* (the emphatic, not the connective, negative—SS 640–1). The first element in *nekku* is in that case to be identified with the IE negative *ne-, with which Pedersen believes the *na*- of Hit. *natta* corresponds 'gegen die gewöhnliche Regel' (MS 71), though Sturtevant's explanation of *natta* as derived from an IH form *nb* (HG 132) is, in my opinion, more satisfactory. *Nekku* is found in KUB 24.8.2.16–18 KUR-*e-wa ni-ik-[k]u ku-wa-*

However, there are possible objections in the way of an explanation that assumes an original close connection between *ta* and *takku*.

(1) Although, as has been said, both words belong to the early speech, their subsequent history is far from parallel, for the suppression of *ta* by *nu* was both earlier and more complete than that of *takku* by *man*. This is made clear by an examination of the two earliest documents in a sufficiently well-preserved state to throw light on syntactic problems: the Law Code and the Proclamation of Telepinus. *Ta* is used frequently in the early parts of the Code (§§1–100, 101–200), but less frequently than *nu*[16]; in the later version of the Code (§§I–XL) it appears only twice[17], and in Tel. not at all. On the other hand *takku* is the regular word for 'if' in the Code, occurring at least once in very nearly every paragraph, while *man* in the sense of 'if' is rare, being found only ten times in the older version[18], though in the later one it becomes commoner[19]; in Tel. the distribution is almost equal, with four instances

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pi-ik-ki ḫar-kán ma-an-wa URU-AŠ.AŠ.ḪI.A *ni-ik-ku ku-u-wa-*[*p*]*i-ik-ki dan-na-ti-eš-ša-an-zi ma-a-an-wa* ᴸᵁERÍN.MEŠ *ni-ik-ku ku-wa-pi-ki ḫu-ul-la-an-te-eš*, which I think means ' "the country is not anywhere destroyed; it may be that they are not anywhere deserting the cities, it may be that the troops are not anywhere defeated" '. (*Man* here seems used, like Gr. ἄν, to express potentiality; if I am right, the passage belongs with those cited by Friedrich in KlF 1.288–289.) I owe my knowledge of the existence and the occurrence of *nekku* to Professor Goetze, who, however, does not interpret the *man* clauses as I do. But I am pleased to learn from a letter from him that my view, independently reached, concerning the origin and meaning of *nekku*, is confirmed by his having arrived at the conclusion that the word is a negative derived from *ne*- and -*aku* (I would rather say *ne*- and -*ku*). None the less, the passage is so hard to understand that I make suggestions concerning it only with the utmost diffidence.

[15] It is true that the meaning of -*ku* -*aku* is regularly 'or' not 'and', but the difference between 'or' and 'and' is frequently merely a matter of idiom. Note that Gr. εἴτε ... εἴτε corresponds to Lat. *sive* ... *sive* and Eng. *either* ... *or*, *whether* ... *or*; that Gr. οὔτε ... οὔτε, Lat. *neque* ... *neque* correspond to Eng. *neither* ... *nor*; and that Lat. *neque* and *neve* have precisely the same connective value (on the latter word cf. the comment in EM 1036, 'la disjonction équivaut souvent à "et" '). The distinction between 'and' and 'or' seems especially likely to fade out in cases of gemination, such as those cited here from Gr., Lat., and Eng.; it is doubtless significant that in all the occurrences of -(*a*)*ku* in the Code (§§ 1, 2, 19, 98) it is doubled.

[16] For occurrences of *ta*, see note 4. *Nu* is found *passim*.

[17] §§ XXXIV and XXXVIII.

[18] §§ 5, 40, 41, 49 (line 54), 50, 53 (bis), 71, 171, 193.

[19] Though this portion of the Code is only about a fifth as long as the older part, conditional *man* is met twelve times here as against ten times there. There seems to be a special tendency to use it in new additions, often side by side with *takku* in

of *takku*[20] and six (not all certain) of *man*[21].   In classical Hit., so far as
I know, *ta* is never met; but *takku* still occurs (though rarely), e.g., in
KUB 13.3.3.18, where it follows *man* (14) apparently with no differ-
ence in meaning.   However, this difference in the fate of the two words
of course need not betoken a difference in origin; in classical Lat. *dum*
except in a few specialized and isolated cases ceased to function as an
independent adverb, but was widely used as a conjunction.

(2) There is no indication of a particular fondness on the part of *ta*
and *takku* for each other's company.   On the contrary, they seem ac-
tually to avoid each other[22].   The Code reveals this in two ways.   (a)
In the case of successive connected protases ('if . . . and'), after *takku*
'and' is rendered by *ta* only 19 times[23] and by *nu* 41 times[24], but after
man by *ta* twice[25] and by *nu* not at all[26].   However, the examples with
man are too few to be significant.   (b) In cases where a sentence con-
nective is used to introduce the apodosis[27], *takku* is followed by *ta* only 7
times[28], but by *nu* 32 times[29].   However, this need not indicate any

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portions retained from the older version; see §§ VII, X (bis), XI (Hrozný makes
man temporal in these three sections, but I think he is wrong), XXXIV, XXXV
(bis), XXXVI, XXXVII (three times), XXXIXA (note *takku* in VII echoing 7
and 8, in X echoing 11, in XI echoing 12, in XXXVI and XXXIXA echoing 47A,
and in XXXVII echoing 47B).

[20] 2.36, 55, 4.20 (bis).

[21] 2.38 (where it is used as a parallel for *takku* in 36), 51, 59, 70, 4.13, 16.   Some
of these instances may be temporal; e.g. Sturtevant (Chr. 191) so interprets *man*
in 2.59.   Temporal *man* is commoner than conditional *man* in Tel.; there are ten
sure examples (1.8, 18, 21, 24, 58, 63, 2.4, 16, 20, 27).   Finally, there are two other
instances of *man* (3.74 and 4.7) in passages so fragmentary that we cannot judge
its force.

[22] For instance in §171 we find side by side *nu* following *takku* but *ta* following
man.

[23] §§ 10, 43, 53, 60, 61, 62, 86, 107, 124, 144, 146, 163, 164, 169, 174 (cf. note 48),
193, 195 (bis), 197.   There are no cases at all in the later version.

[24] §§ 4, 19 (bis), 20, 21, 22, 23 (bis), 27 (bis), 29, 30, 31, 34, 36, 38, 40, 41, 42, 43,
44, 45, 71, 75, 77 (bis), 78, 84, 86, 87, 88, 89, 90, 105, 106, 108, 149, 163, 171, 191, 198.
In addition there are two instances in the later version, X and XI; but in my
figures I am disregarding this version, as it almost always prefers *nu* to *ta* in
every usage (cf. sup.).

[25] §§ 53, 171.

[26] Except for one case in the later version (XXXV).

[27] Asyndeton is overwhelmingly the rule in the Code.

[28] §§ 46, 103, 162, 164–5, 172, 196, 198; also XXXVIII.

[29] §§ 10, 11, 19, 20, 21, 22, 28 (bis), 34, 35, 36, 40, 41, 43, 44, 45, 70, 71 (three times),
75 (bis), 86 (bis), 94, 95, 99, 100, 162, 175, 198, 200; also IV, VIII, XI, XXXV,
XXXVIII.

special aversion to *ta* on the part of *takku*, since approximately the same ratio prevails after *man*[30], which is followed by *ta* in apodosis once[31] and by *nu* 5 times[32].

Hence the second possible objection to the theory seems to grow out of the first, and not to constitute a separate obstacle. If *ta* is not so common with *takku* as the hypothesis of their original intimate connection might lead us to expect, it is simply because *ta* is not so common as *nu* under any circumstances, and is growing increasingly less common. Could we reach earlier linguistic evidence than we possess[33], we might well expect to find the use of *ta* with *takku* far more widespread.

However, those who desire or demand an explanation for *takku* not involving *ta*, may prefer to trace the first element directly to the demonstrative stem, comparing Gr. $\epsilon \iota$[34] and Eng. *if*[35] from the demonstrative stem **e-*, **i-*, Lat. *si* from the demonstrative stem **so-*[36]. With the second element may be compared the similar use in conditions of Skt. *-ca* and Lat. *-que* in *absque*[37], possibly also old Eng. *an, an if,* really *and, and if*[38].

3. *tamais*

I should like to suggest the possibility that the demonstrative pronominal stem which in my opinion is seen in *ta* and *takku* may also be the basis of *tamais* 'other'[39]. If I am right in this, the Hit. word for

[30] Of course only conditional *man* is referred to (with temporal *man* we find only *nu*—in §§ 10, 55, also IX—, never *ta*). After a relative clause the use of *ta* is proportionately more extensive, *ta* occurring three times (§§ 28, 48, 167) and *nu* six times (§§ 23 bis, 28, 30, 98, 106; in addition IV, XXXIV, XXXV, XXXVII, XXXIX, XL bis); but once more the numbers are not large enough to be impressive.

[31] § 171. Even this one instance is not certain, since possibly the apodosis does not begin until *nuza* in line 8.

[32] §§ 5, 40, 41, 50, 71 (the last two instances are doubtful); also X (bis), XI, XXXIV, XXXV (bis), XXXVI.

[33] On the degree of archaism still existing in the language of the Code and Telepinus, cf. Sturtevant, Chr. 224 and 194 respectively.

[34] WP 1.99.

[35] Probably. For the Germanic forms see WP 1.98.

[36] SS 284, 771; EM 907; WP 2.458.

[37] Doubtless to be associated, as they are by Br. 706 and Walde 5 (though Hofmann dissents, SS 531), whether we regard *-que* as emphatic with Br. (706–7) or as connective with Hofmann (SS 531) and EM (3).

[38] New Eng. Dict. 1.208.

[39] The possibility that *tamais* is derived directly from *ta* would seem to be precluded by the fact that *ta* always has initial position.

'other' is parallel in its development to the IE words in general, which are derived from various demonstrative stems, as follows: (1) *al-, *ol- (Gr.ἄλλος; Lat. *alius* and *alter*; Goth. *aljis*, Eng. *else*; OIr. *aile*; Arm. *ail*)[40]; (2) *en-, *on- (Skt. *anyás* and *ántaras*; Goth. *anþar*, OHG. *andar*, Eng. *other*; Lith. *añtras*, OPr. *antars*)[41]; (3) *i- (Skt. *ítaras*)[42].

Sturtevant and Pedersen associate *tamais* with *tan* 'for the second time' (found also in the phrase *tan petas* 'of second rank') and *tayukas* 'two years old', in both of which words they see the root meaning 'two' (Hg 144, 202; MS 68). The by no means self-evident connection Sturtevant explains (HG 128) by equating *ta-* with *de-*, the prior element of *dewe*, the ablaut base of *dwōu*. To me it seems at least as likely that *ta-* shows the ordinal numeral stem. In the IE languages this stem in the case of the number two shows a marked tendency to be different from the cardinal, and at the same time to be associated with the word meaning 'other' (which itself ·is regularly, as has just been pointed out, connected with a demonstrative stem, never with a cardinal numeral). Note that of the ancient forms for 'second' listed by Br. (390–391), only Skt. *dvitíyas* resembles the cardinal, while the great majority resemble the word for 'other', as follows: (1) Lat. *alter*, OIr. *aile*; (2) Goth. *anþar*, OHG *andar*, Lith *añtras*; (3) Lat. *iterum* 'for the second time', to be compared with Skt. *itaras*.

Hit. *tan* (in meaning identical with Lat. *iterum*) is obviously an ordinal; and I believe the same may be true of *ta-* in *tayukas*[43], since in

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[40] Br. 424; WP 1.85–86; Walde 30. On *alius* in particular see Sommer, IF 11.2–3; 24.24–5; Brugmann, IF 24.162–3.

[41] Br. 424; WP 2.337; Walde 405.

[42] Br. 424; WP 1.99.

[43] There is a serious obstacle to this view, if Pedersen is right in his tentative suggestion (MS 68) that *tayukas* is parallel to Lith. *dveigỹs*, which is clearly from the stem *dwō(u) (WP 1.819). But our lack of certain knowledge concerning the second element of both *tayukas* and *dveigỹs* renders the matter highly dubious. In regard to *tayukas*, Sturtevant says (HG 144) that 'our ignorance of the etymology and primary meaning of the second member makes its classification impossible'. In this connection it may be pertinent to note that this member stands alone as an independent word, *yukas* 'one year old'; there seems to be no parallel for this in Lith. As for *dveigỹs*, Pedersen in order to bring it into connection with the Hit. form in -*yuk*- is obliged to assume the loss of a vowel before the *g* in the Lith., thus departing from the usual explanation of it as a multiplicative in -*gh*- akin to Gr. δίχα (J. Schmidt, KZ 16.439–40; Felix Solmsen, PBSB 27.358; Gr. 2.1.513). No matter what view we adopt concerning the final element, it would appear obvious, as is pointed out by both Schmidt (440) and Solmsen (358–9), that in the type *dveigỹs, treigỹs, ketvérgis*, as in the Lat. *sexagenarius*, the idea

the Code §§57 and 58 *yukas* and *tayukas* are better parallels for *sawetesza* 'of the same year' (i.e. the year of birth) if they mean 'of the (first) year' and 'of the second year' (i.e., having completed the first or the second year) than if they mean 'one year old' and 'two years old'. It may be urged against me that *yukas* and *tayukas* (on which see HG 144) are not compounds of precisely the same sort as *sawetesza* (on which see HG 146); but that they have a similar meaning is made clear by the ideographic writing MU 1 and MU 2, §§60, 61, 63, 67.

The alternative spelling (about equally common, I am told by Professor Sturtevant) of *tamais*, *tan*, and *tayukas* with initial *d* is of no importance, since the Hittite scribes regularly confused *t* and *d*[44]. Still, it may be due in part to the influence of the cardinal numeral for two, which we may safely assume is connected with the stem *$dw\bar{o}(u)$.

The latter part of *tamais* has not been explained. I should like to venture the suggestion that we have here added to the root *ta-* the suffix *-ma-*, corresponding to the IE suffix *-mo-*, seen in ordinal numerals and superlatives[45]. This explanation is applicable whether we recognize in *ta-* a demonstrative stem, as I have proposed, or a numeral stem, as Sturtevant and Pedersen believe. In the former case, cf. the addition of the suffix *-tero-*, seen in ordinal numerals (as Gr. δεύτερος, Germ *zweiter*) and comparatives, to a demonstrative stem in Skt. *itaras*, Lat. *alter*, Goth. *anþar*[46]. In the latter case, cf. Lat. *septimus*, *decimus*.

That Hittite had the suffix *-ma-* is proved by *septamiya*, presumably from a nominative *$septamas$[47]. A combination of the root *ta-* with the same suffix should give us a nominative *$tamas$[48]; but this may have become *tamais* under the influence of *hantezis* 'first' and *apezis* 'last', thus passing from the *a*-declension to the *i*-declension. We

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of AGE is only secondary; cf. Lith. *ketvergas*, Rus. *četvêrg* 'Thursday', i.e., the fourth day of the week (see Solmsen 358 and Gr. 2.1.513 for the Rus. parallel, and, in regard to both words, Franz Miklosich, Denkschr. 24.20). This fact would hardly accord with the independent existence in Hit. of the simple *yukas* already referred to.

[44] HG 65-7, 127-8.

[45] Br. 339, 340, 346-7.

[46] Br. 339, 390-1.

[47] HG 156.

[48] A form *tamas* (*ta-ma-aš*) is actually printed by Hrozný in Code § 174 *ták-ku* LÛ.MEŠ *za-aḫ-ḫa-an-da ta-ma-aš a-ki*, 'if men fight (and) one dies'. But Professor Goetze has suggested to me by letter that the correct reading here may be *ta* l-*as*, which seems far more plausible.

might rather expect *tamis*; but for the addition of -*is* to the stem
vowel -*a*, cf. the addition of -*is* to the stem vowel -*u* in *parkuis* beside
parkus and *tankuis* beside *tankus*[49]. In other words, *tamais* : *tamas*
= *parkuis* : *parkus*. If *tamais* was pronounced as a trisyllable, the
formation may have been due to a desire to give it the same length as
hantezis and *apezis*; but I think we should rather assume concerning
the *a* and *i* that they belong in the same syllable, as Sturtevant does
concerning the *u* and *i* of *parkuis* and *tankuis*, which he spells *parkwis*
and *tankwis*.

The formation of *tamais* instead of *tamis* may have been aided by the
existence of such forms as *zahais* beside *zahis*, etc.[50] I am not suggesting
that the -*ai*- forms *tamais*, *tamain*, *tamai* belong with the types *zahais*,
zahain, *hastai*, since these particular forms seem to be confined to nouns[51];
as I have indicated, I account otherwise for the -*ai*- in *tamais*. But
certainly our adjective may have been affected by the general con-
fusion among the *i*-stems of full grade and lengthened grade forms[52];
and at least in the nominative and accusative masculine plural, *tamais*
resembles markedly the -*is* adjectives such as *tankwis* and *parkwis*,
which tended to go along with the lengthened rather than the full grade
forms[53]. If it be asked why *tamais* (i.e. *tamis*) should do what *hantezis*
and *apezis* do not, the answer is perhaps to be found in a statement
which I have heard made by Professor Goetze, to wit that there is a
tendency among the LONGER adjectives[54] to show no vowel variation.

Tamais, however, is not declined throughout like either *hantezis* or
parkwis. After the manner of Latin *alius* and *alter* it borrowed certain
forms from the pronominal declension, namely the genitive and the
dative, both singular and plural[55], and in addition acquired a peculiar
ablative singular, *tametaz*, which seems to present a combination of the
-*et*- seen in the pronominal declension (e.g. dative *ketani*, *tametani*)
with the ablative termination -*az* found regularly throughout the noun
declension[56]. Except for the forms just enumerated, *tamais* in declen-
sion follows the model of the -*is* adjectives.

[49] Sturtevant, LANG. 10.268.
[50] HG 103, 147, 180.
[51] See the paradigms in HG 180.
[52] Cf. HG 179 note 89.
[53] HG 179.
[54] E.g. *karuwilis*, *tarhuwilis*, *hantezis*, etc.
[55] See the paradigms in HG 207-11.
[56] HG 206.

4. *takiya*

The stem seen in *tamais* undoubtedly, in my opinion, reappears in another word that I think also means 'other'. This is *takiya*, which I would explain as a dative-locative compounded of *ta-* and *-kas*. The second element I believe is to be identified with *kas* the demonstrative pronoun[57].

Kas has not been met as an enclitic, but this fact need not be considered an insuperable obstacle. The particle *man* is found both as enclitic and as non-enclitic, the two uses being apparently interchangeable[58]. The personal and the demonstrative pronouns have both enclitic and non-enclitic varieties, again seemingly without difference in meaning; cf., e.g., *-as* and *apas* as used in KBo 6.34.1.18–19[59]. Friedrich suggests (ZA NF 3.194), though not with certainty, that *apas* itself may be enclitic in KBo 4.6.1.13. On the other hand, the neuter nominative-accusative plural of *-as*, regularly enclitic *-e*, is non-enclitic in the combination *esta*, in KBo 3.28.2.5, 3.38.1.15[60].

A possible parallel instance of a pronominal form containing *kas* as its second element is *nakkus*, seen in Code §98 *na-ak-ku-uš Ú-UL šar-ni-ik-zi*. The passage has been translated as follows: Hrozný, '*tels* il ne restitue pas'; Zim.-Fried., 'dafür braucht er nicht Entschädigungen zu leisten'; Walther, 'nothing of these he need compensate'. Thus all these scholars make *nakkus* a demonstrative; none of them, however, offers any explanation of the form. Attempts have been made to connect it with the adjective *nakkis*, either as nominative singular[61] or as accusative plural[62]; but a *u*-form from *nakkis* is troublesome. I suggest that *nakkus* may be a compound of *nu* and *-kas*[63], the *na-* by analogy

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[57] From the stem **ke-/ *ko-/ *ki-* seen in Gr. κεῖνος, Lat. *cedo* and *cis*, etc. (HG 201).

[58] See Friedrich, KlF 1.286.

[59] Published by Friedrich in ZA NF 1.161 ff.

[60] Goetze apud Sturtevant, Gl. s.v. (For the form *-e*, See HG 198, 206.)

[61] Götze, Hat. 94 note 5. Here 'der Schuldige' is offered as a translation. But concerning this Professor Goetze has written me as follows: 'The explanation for my "der Schuldige" comes in part at least from KBo 5.2.3.30 (cf. OLZ 1925.237), where I read *na-ak-ku-uš ŠA ḪI-TI*. It is now sure that this reading was wrong and that we have to deal with a Hurritic word. So I agree today with everybody who regards *nakkus* in the passage from the Code as the sentence connective with pronominal elements.'

[62] HG 176. But Professor Sturtevant tells me he accepts my suggestion with regard to the form under consideration.

[63] Professor Goetze writes me that he prefers to view it as a compound of *nu* and *-aku* and *-as*. This has the advantage of explaining the *na-* without recourse to analogy; but I do not see how to make it fit the sense.

with *nas*, possibly also to some extent with *takkus* (found in the Code e.g. §§197, 198), which is similar in form though not in formation. I translate 'these he need not pay for'.

The addition of -*kas* to another demonstrative stem, as presupposed by my explanation of *takiya*, has a parallel in the corresponding employment of -*ce* seen in Lat. *hic*, *illic*, etc.[64] It is true that -*kas* is an inflected and -*ce* an uninflected form of the demonstrative stem, but this distinction is not of significance; inflected and uninflected forms of the indefinite stem are used without differentiation in Lat. *quisquis* and *quisque*, which in early Lat. are interchangeable[65], and of these two it is the inflected one that is added in *aliquis* to *ali- from *al-[66], which, as has been pointed out above in connection with *alius*, would seem to be a semantic parallel to the *ta*- in *tamais*.

There may seem to be a serious difficulty in the attribution to a pronominal form of a dative-locative termination that belongs to the noun declension. But such confusion is not unparalleled[67]. Though the dative-locative ending of the pronouns normally consists of formative -*t*-[68] plus -*ani*, for -*ani* may be substituted -*i* or -*a*, both of which belong to the noun declension[69] (cf. *keti* from *kas* and *apeta* from *apas*[70]), and *kas* has beside the regular *ketani* and the just-mentioned *keti* a peculiar form *ketaniya*[71], which seems to be a composite of the pronoun ending -*ani* and the noun ending -*iya* or possibly -*a*[72]. Moreover, an admirable parallel is provided by the numeral 1-*as*, which, according to the convincing evidence just presented by Götze in Lang. 11.185–90, is almost assuredly to be read *sanas*[73]. The word, like Lat. *unus*, is declined

[64] SS 286.

[65] See the present writer, TAPA 64.32–40.

[66] Sommer, IF 11.5–6; 24.17–24; Brugmann, IF 24.160–3; Sommer, Hdb. 448, Krit. 69.

[67] Similarly, Lat. *alius* shows a dative *alio*, *aliae*, *alio* (SS 291, EM 32), and there are plenty of other cases of comparable borrowings by pronouns and pronominal adjectives from the noun declension, both in Lat. (SS 291) and in Skt. (Whitney 199).

[68] HG 204.

[69] HG 205.

[70] HG 208.

[71] HG 208.

[72] However, Professor Goetze suggests that the final element may be -*ya* = Lat. *que*.

[73] Götze spells it *sannas*; but v. inf., note 76.

after the manner of the pronouns, with genitive 1-*e-el* and dative-locative 1-*e-da-ni*[74], i.e. *sanel* and *sanetani*, corresponding to *kel* and *ketani* from *kas*[75]. But Götze (loc. cit. 190 note 12) suggests that the phrases '*ša-ni-ya ú-it-ti* and *ša-ni-ya ši-wa-at* 2BoTU 7 10 and 60 resp. possibly contain our word'; that is, *sanas* has a dative-locative *saniya*[76], corresponding perfectly to our *takiya* from **takas*.

The meaning of *takiya* I believe is made clear by an examination of several passages in the Code.

Hittite says *kas* . . . *kas* 'this one . . . this one' where Eng. would say *this one* . . . *that one*[77]. Examples from the Code are §49 *ka-a-aš-ma-an ku-u-un e-ip-zi ka-a-ša-ma-an ku-u-un e-ip-zi*, 'this man would seize this one, and that one would seize that one'; §166 GUD.ḪI.A. *tu-ri-ya-an-zi ki-e-el me-ne-iš-ši-it du-wa-a-an ki-e-el-la me-ne-iš-ši-it du-wa-a-an ne-e-ya-an-zi*, 'they yoke oxen and they turn this one's face in one direction and that one's face in the other direction'; possibly, too, §40 (repeated in §41) *ki-i* GIŠTUKUL-*li-me-it ki-i-ma ša-aḫ-ḫa-mi-it*, if we emphasize the distinction doubtless implied by -*ma* between the first *ki* and the second, as Walther does in his translation, 'This is my military holding, but this (other one) is my tenancy'[78].

We find *kas* . . . *kas* combined with *takiya* . . . *takiya* 'in this (place) . . . in this (place)', i.e. 'in this (place) . . . in that (place)' in §191 *ták-ku* LÚ *EL.LUM a-ra-aḫ-ḫa-an-ni-uš* (erased) *an-na-ne-ku-uš an-na-aš-ma-an-na ú-en-zi ka-a-aš-ma ta-ki-ya ut-ne-e-*(. . .?) *ka-a-aš-ša ta-ki-ya ut-ne-e-ya*, 'if a free man has intercourse with (women of some

[74] Götze, LANG. 11.190.

[75] HG 208.

[76] As a variant of this showing a different borrowing from the noun declension, I would offer *sane* found in the phrase *sa-ni-e pí-di* 'in one place', heretofore written *SA-NI-E pí-di* and not satisfactorily explained. (Cf. Sommer, AU 61 note 6, on KUB 12.50.10, which he characterizes as 'unklar'. A second occurrence of the phrase, KBo 4.9.1.15, has been called to my attention by Professor Sturtevant.) The fact that *sane* resembles *saniya* and *saneta* (on which see Götze, LANG. 11.190) in being spelled with one *n* helps suggest that we should write *sanas* rather than *sannas*. This has been pointed out to me by Professor Sturtevant, who writes: 'All direct evidence points to *sanas* with one *n*; *sannapi* and *sannapilis* indicate *nn* only for derivatives.'

[77] Cf. the similar use of Lat. *hic* . . . *hic* (SS 475); also the repetition of the same stem in Fr. *celui-ci* . . . *celui-là*.

[78] I prefer Sturtevant's version (Chr. 221): 'This (is) my military service, and this (is) my service to the landlord'.

specified sort[79]), these[80] (*kas*) in this (*takiya*) country and those in that',
and again in §196 *ták-ku* ERUM.MEŠ.ŠÛ GÌM.MEŠ.ŠÛ . . . *tu-uš
a-ar-nu-wa-an-zi ku-u-un-na ta-ki-ya* URU-*ri ku-u-un-na ta-ki-ya* URU-
ri a-še-ša-an-zi ki-e-el 1 UDU *ki-e-el-la* 1 UDU *ka-aš-ša-aš ḫu-u-it-ti-ya-
an-ta*, 'if (any one)'s male and female slaves (have intercourse), they
(indefinite, French *on*) take them and settle them this one in this city
and that one in that; a lamb is brought as this one's substitute, and a
lamb as that one's'.

From this distributive use 'in this (place) . . . in that', i.e. 'in one
(place) . . . in another', *takiya* used alone could readily acquire the
meaning 'in another place'[81].   I think that is without doubt its sense
in §6 *ták-ku* LÛ.GÀL.LU-*aš na-aš-ma* SAL-*za ta-ki-ya* URU-*ri a-ki*,
'if a person, man or woman, dies in another city'.   Hrozný's 'dans une
ville *quelconque*' and Walther's 'in this or that village'[82] seem point-
less; but 'in another city', the translation given by Sturtevant[83] appears
to make excellent sense; note that in the preceding paragraph, 5, pro-
vision has been made for a certain contingency according to whether it
occurs in Hatti or elsewhere[84].   Moreover, we must observe, as Sturte-
vant (Chr. 224–5) reminds us, that *takiya* URU-*ri* in §6 corresponds,
even though only roughly, with *tamedani* A.ŠAG in the later variant,
§IV.

'Foreign' ('fremd', Zim.-Fried.) conveys the same idea[85]; 'distant'
(Götze, ap. Sturtevant, Chr. 225)seems to me slightly less apt, yet it
may be pertinent to note that the same word (*pára*) means 'other'
and 'distant' in Sanskrit[86].

[79] I am not endeavoring to render *an-na-ne-ku-uš an-na-aš-ma-an-na* (a phrase
which occurs also in §194).   Hrozný's 'celles-ci et celles-là', Zim.-Fried.'s '*dieser
und jener*', and Walther's 'now this one, now that one (?)' are mere guesses.

[80] I am assuming that *kas*, like *nas* and *tas*, may be accusative plural, though
HG (208) does not so list it.   If it may not, we shall have to translate 'this man
in this country, that one in that'.

[81] I am gratified to find that in developing this idea I have used the same line
of reasoning as that employed by Brugmann (IF 24.160–2) with reference to Greek
and Latin.

[82] I presume Walther bases this interpretation on §§191 and 196.

[83] Though with reservations; see his note, Chr. 224–5.

[84] Contrast §XXXVII, which states that the same policy holds good *ma-a-an
a-pi-e-da-ni ut-ne-e ma-a-an da-me-e-da-ni* KUR-*e*, 'whether in this country or in
another country'.

[85] It will not do, however, according to my view, in §§191 and 196, where they
also use it.

[86] Whitney 199.

THE NAME *Phoinikes*

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The purpose of the present paper is not to advance yet another etymology for Greek Φοῖνιξ 'Phoenician', but rather to reduce the number of acceptable suggestions with the aid of new evidence from an independent quarter. Although the material to be considered is predominantly linguistic, archaeological sources will also be brought into discussion.

The current explanations of the ethnicon Φοῖνιξ and its immediate relatives fall into three groups,[1] as follows:

1. The term is Greek in origin, going back to φοινός 'blood-red'.
2. It is based on Egyptian *Fnḫ-w* 'Phoenicians'.
3. Both Φοῖνιξ and *Fnḫ-w* go back to a common source.

Let us first re-state briefly the arguments used by each of the above factions.

1. φοῖνιξ is a genuine Greek word with numerous and diversified usages. It signifies the fabulous bird 'Phoenix', 'red purple', 'palm tree' and its fruit the 'date', and 'a musical instrument'. All these terms may have had a common origin, on the assumption of some ultimate connection with Phoenicia, or the Phoenicians. But the same can scarcely be true of the numerous instances in which Φοῖνιξ appears as a proper name outside Phoenicia. Thus it is found to designate a river near Thermopylae, a mountain in Boeotia and in Caria, a god and a place in Crete, not to mention the derivative formations of this term.[2] Etymologically we have here the adjective φοινός 'blood-red', with the suffix *īk*, a perfectly normal construction according to W. Schulze.[3] There remains the problem of justifying the connection between 'Phoenician' and 'red'. On this point the arguments become colorful in-

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[1] A complete statement of the various views would require a lengthy article. With one or two exceptions, only the latest discussions of the problems at issue will be cited below.

[2] Cf. Eduard Meyer, Geschichte des Altertums I² 1.97; 2. 66.

[3] Berlin SB 1910, 803 f.

deed. The Phoenicians earned this distinction by being red-skinned;[4] they owe the name to the circumstance that their land, in common with Caria, was noted for red skies in the morning;[5] they were so named because of their far-famed ability to extract from murex shells a red-purple dye.[6]

2. *Fnḫ-w* is one of several names by which Phoenicians were known to the Egyptians. This became Φοίνικ-ες in Greek, and the sing. form was subsequently applied to such Phoenician articles as the palm, the musical instrument in question, and the purple dye.[7]

3. An exhaustive examination of all the passages in which *Fnḫ-w* occurs was made by the distinguished German Egyptologist Kurt Sethe.[8] He found that the bearers of that name were localized generally in Palestine and Syria; specific references to Phoenicia proper are clear in the Ptolemaic period. The name may perhaps be traced to the word *fnḫ* which occurs as early as the Old Kingdom and means something like 'skilful', or as an appellative 'carpenter, shipwright'.[9] The specialization for 'Phoenician' would not be surprising in view of the known proficiency of the Phoenicians in the art of building ships. Since the Greeks appear to have associated the Phoenicians with another local industry, viz. the production of red purple, there is a likelihood of ultimate relationship between the Greek and the Egyptian designations. But a direct connection is precluded by phonetic considerations, the respective initial consonants (φ and f) not being normally interchangeable.[10] The difficulty would disappear if we assumed an original Phoenician term from which both Φοίνικ-ες and *Fnḫ-w* were derived. Popular etymology later modified this assumed prototype into 'maker(s) of red purple' and 'shipwright(s)' respectively. To be sure, no such native name has been discovered as yet; but available Phoenician sources are rather scanty.

[4] First suggested by Pietschmann, Geschichte der Phönizier 107.

[5] Beloch, Griechische Geschichte I[2] 70.

[6] Meyer, op. cit.

[7] First proposed by Brugsch, Geschichte Ägyptens unter den Pharaonen 242.

[8] Der Name der Phönizier bei Griechen und Ägyptern (Mitteil. d. Vorderas. Ges. 21. 305 ff).

[9] It is interesting to note that virtually the same accomplishments are attributed to the divine artificer of the Semitic inhabitants of Ugarit; cf. Ginsberg, JRAS 1935. 49f.

[10] As a matter of fact, the only sound that the words have definitely in common is [n]; the vowels of the Greek ethnicon would be strange in a Semito-Hamitic word, but we are spared the necessity of further comparisons thanks to the normal Egyptian practice of vowelless writing.

It will be evident even from this summary presentation that each of the above positions is open to serious criticism. To begin with the last one, Sethe crystallized for us the meaning of the term *Fnḫ-w* in all its ramifications. But in assuming an ultimate dependence upon a native Phoenician name he was building on an argument from silence. He was not blind, however, to this inherent weakness of his theory, which can not be said for his many excerptors.[11] In 1916, the year in which Sethe's study appeared, it was not unreasonable to expect the discovery of some Asiatic prototype of *Fnḫ-w*. Since then we have had, however, an enormous increase in Phoenician and other West-Semitic epigraphic materials. They have failed to affect the onomastic situation. The only known native name for the entire district remains as before 'Canaan' (Phoenician and Hebrew *Knʻn*, cuneiform *Kinaḫḫi, Kinaḫna*),[12] while the people called themselves after this or that leading city (Sidon, Gubla/Byblos, Ugarit, and the like). The second view, viz. the importation of the Greek term from Egypt, betrays no worry over phonetic considerations. For this very reason Sethe himself found it untenable;[13] nor has anyone succeeded in solving the puzzle of why the Greeks should have combed Egypt in search of a suitable designation for the Phoenicians. Moreover, *Fnḫ-w* was neither the commonest nor the least ambiguous Egyptian appellation for these Asiatics or their country. Any thought of a connection between Φοίνικ-ες and *Fnḫ-w*, must therefore be given up.

We are thus back to the first interpretation of the Greek term as a strictly European development. That is to say, φοίνιξ started out as a Greek appellative, presumably based on φοινός 'red.' Now we can not be concerned at present with all the usages of this term. Some of them may have had the same underlying basis,[14] and others may have arisen independently, entirely plausible alternatives once the color red has been allowed as a starting point. But how did the Phoenicians

[11] Cf., e.g., Peiser, OLZ 1919. 5 ff., who would see in *Kinaḫḫi* 'Canaan' the prototype required by Sethe, and Eisler, ZDMG 1919. 154ff. I am obliged to my colleague Dr. Z. S. Harris for calling my attention to several discussions on the subject.

[12] See Index to Knudtzon's Amarna 1577. The origin and etymology of *Knʻn* and its cognates are not strictly relevant to the present problem.

[13] See his explicit statement, op. cit. 329: 'Von einer direketen Abhängigkeit ... kann selbstverständlich keine Rede sein.'

[14] E.g. the name of the fabulous bird 'Phoenix' may have been linked with the adjective and the ethnicon under the influence of *byn-w*, the Egyptian designation for the legendary bird; cf. Sethe 307.

come to be regarded as 'Reds'? Disregarding all fanciful attempts at explanation, we are left with a purely industrial set-up. φοῖνιξ came to mean 'Phoenician' because the word signified 'red purple' and the dyer producing this color, the Phoenicians being proficient in this form of work. This theory found a very vigorous champion in the late Eduard Meyer. It has considerable support in Greek tradition as far back as the Homeric sources, and it is in accordance with the testimony of later classical writers.[15] The principal difficulty arising from such a view is that it presupposes the naming of a people after one of its products instead of the other way about. Recent archaeological discoveries may help to obviate this particular objection. But before material sources are drawn upon, I wish to adduce another type of evidence, capable of establishing the intimate association of Phoenicia with the purple dye industry beyond any possibility of dispute.

In the cuneiform records from Nuzi, in the East-Tigris area, which date from the middle of the second millennium B.C., there occurs the adjective *kinaḫḫu* in passages dealing with wool. One published text simply uses the word as descriptive of wool.[16] Another one permits us to arrive at a closer definition of the term involved; it reads as follows: *bi-ir-me-šu-nu ša ku-zi-ti ša ki-na-aḫ-ḫu ša ta-wa-ar-wa* 'the dyes of the cover (are) of *kinaḫḫu* (and) of *tawarru*'.[17] The latter word (usually found in the form *tabarru*; the final *wa* in the present instance is the Hurrian genitive ending) is known to designate 'deep yellow' and 'red'.[18] In *kinaḫḫu* we have then an adjective descriptive of some dye, presumably of a kind similar to the *tab/warru*-dye. Unpublished texts in the possession of the Semitic Museum at Harvard help further to define *kinaḫḫu* as a sub-variety of *tabarru*, hence 'a kind of red'. Other passages link *kinaḫḫu* with *uqnu* 'lapis, purple'.[19] The combined result of these disclosures is that *kinaḫḫu* signifies '(red-) purple dye'.

We have seen that the proper name *Kinaḫḫu* represents our 'Canaan'. The present adjective, unknown outside the Nuzi texts, is based obviously on this geographic term. To place this equation beyond a shadow of doubt, we have still another Harvard document which

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[15] On purple, especially the Tyrian kind, see Pliny 9. 60–63.

[16] For the text see Chiera, Joint Expedition at Nuzi 125.5.

[17] Ibid. 314. 4 f.

[18] Cf. Meissner, Beiträge zum Assyrischen Wörterbuch 1. 47f.

[19] I am grateful to Prof. R. H. Pfeiffer and to Dr. E. R. Lacheman for enabling me to examine these texts in transliteration kindly furnished by them.

shows that the *kinaḫḫu*-dye was actually imported from the West.[20]
It follows that the land-name 'Canaan', the only one to be applied by
the Phoenicians to their own country, had become in Mesopotamia an
adjective meaning 'purple dye' as early as 1500 B.C. The fame of the
Syrian coast as the home of such dyes antedates thus by centuries the
oldest Greek references that point in the same direction.

Thus far we have had comparatively clear sailing. The use of geo-
graphic terms to describe local products is quite normal. In the case
of Φοῖνιξ, however, the reverse process has to be assumed. Does it
mean that the Greek word for 'red purple' must be derived from the
ethnicon Φοῖνιξ after all? In that case the latter would be left with-
out any etymology, the equation with Egyptian *Fnḫ-w* being definitely
out. Now the cuneiform evidence strongly favors a connection between
the names for the people and their product. Such a connection can be
maintained for Greek it we start out with 'red purple' (based on φοινός
'blood-red') and proceed thence to the Φοῖνικ-ες, but not vice versa.
Or have the two Greek terms really nothing in common, being merely
homonyms? In view of the cuneiform parallel this would be putting
an entirely unwarranted strain on the long arm of coincidence. The
only available solution is to derive the Greek ethnicon from the word
for 'red purple'.

Archaeological evidence may step into this breach with some indirect
confirmation. In a suburb of the North Syrian city of Ugarit[21] (modern
Râs esh-Shamrah) the excavators have uncovered traces of workshops
for making purple dye, to judge from the pounded murex shells left
on the spot.[22] Now it is interesting that these workshops were in the
Mycenaean, not the native quarter. In view of this it may be permis-
sible to conjecture that the word φοῖνιξ was brought to Syria by the
Mycenaeans who found the place an excellent source of supply of the
shells required.[23] In that case the land may have been the first to be
designated after the product, Φοινίκη becoming 'land of the purple
dye', while the inhabitants became Φοῖνικ-ες secondarily. But the

[20] This important document which I first read on the spot soon after it had
been dug up by the late Dr. Chiera was kindly collated for me by Dr. Pfeiffer, the
Curator of the Harvard Semitic Museum.

[21] The importance of the purple industry of Ugarit is attested, incidentally,
by a lengthy cuneiform text recently published by Thureau-Dangin; cf. his article
in Syria 15. 137 ff.

[22] Illustrated London News 1935. 712.

[23] For the purple industry in the Aegean area see G. Casson, Phoenicians and
Purple Industry, Antiquary 1913. 328 ff.

main argument in favor of tracing the ethnicon to the name for the dye need not rest on such purely hypothetical grounds.

In conclusion, I may be permitted to venture an explanation of the curious Greek tradition which would derive the Phoenicians from the Persian Gulf.[24] Historically there is absolutely nothing to justify such a view. Do we have here an example of popular learning? The reasoning may have been something like this: 'Φοῖνιξ means "Phoenician" as well as "red". Why? Because the Phoenicians came from the Red Sea ('Ερυθρὰ θάλασσα, i.e. the Persian Gulf)'. Herodotus himself need not have been the guilty schoolmaster. By his time this play on words may have attained to the dignity of tradition.

[24] Herodotus 7. 89.

THE PUNCTUAL AND SEGMENTATIVE ASPECTS
OF VERBS IN HOPI

B. L. WHORF

Verbs in the Hopi language are noteworthy for their very rich and expressive development of verbal aspects and voices. I shall say nothing in this paper of the nine voices (intransitive, transitive, reflexive, passive, semi-passive, resultative, extended passive, possessive, and cessative); and of the nine aspects (punctual, durative, segmentative, punctual-segmentative, inceptive, progressional, spatial, projective, and continuative) I shall deal with only two. It may be noted that there are no perfective and imperfective aspects; in fact Hopi does not in any way formalize as such the contrast between completion and incompletion of action. Its aspects formalize different varieties of the contrast between point-locus and extent-locus of phenomena, indifferently in time or space, or in both. Hopi also has three tenses: factual or present-past, future, and generalized or usitative. Hopi verbs belong to seven classes or conjugations having slightly different inflectional systems. Class 1, the largest and most creative class, contains a few categories not found in the other classes, among them the segmentative aspect.

The simplex of the Class 1 verb is a bare root of the form CVCV, and is in the third person singular intransitive voice, punctual aspect, and present-past tense. The segmentative aspect is formed by final reduplication of this root plus the durative suffix -*ta*, and produces a change in the meaning of the simplex of the following character: the phenomenon denoted by the root, shown in the punctual aspect as manifested about a point, becomes manifested as a series of repeated interconnected segments of one large phenomenon of a stretched-out segmental character, its extension usually being predominantly in one dimension, indifferently of space or time or both. The nature of the change can best be shown by examples.

ha'ri it is bent in a rounded angle

hari'rita it lies in a meandering line, making successive rounded angles (applied for instance to meander patterns in decoration)

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ho''ci it forms a sharp acute angle

pa''ci it is notched

pï'va it is gullied out

hoci'cita it is zigzag

paci'cita it is serrated

pïva'vata it extends in successive gullies and gulches (said of ground)

ca'mi it is slashed inwards from the edge

cami'mita it is fringed, it is slashed into a fringe along the edge

In these and similar examples the phenomenon is such that it requires a rigid or semi-rigid substance for its field of manifestation. When this is the case the punctual intransitive has somewhat the character of a passive[1] and the segmentative shows the phenomenon multiplied along one dimension of space, like a candle-flame between mirrors. In both aspects the phenomenon shows up as an effect established and thereafter retained in the rigid substance, so that we are presented with a static *tableau* of this effect as it is disposed in space.

Suppose, however, that the phenomenon denoted by the verb root is such as to require a non-rigid or mobile substance for its field of manifestation, for example a liquid or a swarm of mobile particles. In that case a deformation of substance such as is denoted by the root will not be a permanent deformation but will result in a vibrative or pulsative agitation of the substance. The intransitive will no longer seem like a passive to our English-speaking standpoint but will be decidedly active, and the punctual will denote one pulse of the deformation or disturbance, while the segmentative will refer to the entire train or field of the vibrations, both as extending in space and as continuing in time. Thus, for instance:

wa'la it (e.g. a liquid) makes one wave, gives a slosh

nö'ŋa several come out (applied to objects or persons)

wala'lata it is tossing in waves, it is kicking up a sea

nöŋa'ŋata it is coming out in successive multitudes, it is gushing or spraying out; applied e.g. to a fountain

Note that with mobile-substance phenomena the segmentative is both durative in time, in contrast to the momentaneous punctual, and extended in space, in contrast to the definitely 'spotted' location of the punctual. Some phenomena are capable of manifestation in both mobile and rigid substances, especially those defined in terms of a cer-

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[1] It is not a true passive because it does not imply any external agent; it is not a static (at least not in the ordinary sense) because it does not imply duration in time; it is not a true active because activity and result are presented as one.

tain type of contour; e.g. *ta'ho* 'it exhibits one wavelike curve, or makes one undulation'; *taho'hota* referring to a mobile substance means 'it is undulating' (for example, a liquid surface, a snake, a shaken rope), to a non-mobile substance 'it is scalloped' or 'it forms a wave pattern'.

But suppose again that the phenomenon denoted by the stem is one resulting from the type of force known in physics as torque (tendency to produce rotation), which in order for any effect to be apparent requires that the substance be a body with at least a certain degree of rigidity and yet capable of certain degrees of motion relative to other bodies.   In this case a single deformation or displacement as denoted by the punctual will be either a single oscillation or a single turning of this body according to the degree of freedom implied in the root-meaning; while if the effect continues it will continue as a train of oscillations or a continued rotation and may or may not involve an advance through space at the same time: this, then, will be the meaning of the segmentative.   Examples of this type of meaning are:

wa'ya makes a waving shake (like a small tree shaken)

ŋa'ya makes a sway from one side to the other

pï·'ya makes a flap like a pair of wings

ta'ya makes a racking shake

ŋö'ya makes a circuit (axial turning combined with advance in an arc)

ro'ya makes a turn or twist

ri'ya makes a quick spin

It is interesting to note that a great many (though not all) these torque movements are denoted by stems ending in *-ya*.   The segmentatives of this type correspond to English durative forms denoting vibrative or rotative motion, e.g.:

waya'yata it is shaking	*pï·ya'yata* it is flapping wings
ŋaya'yata it is swaying	*ŋöya'yata* it is circling round and round
roya'yata it is rotating	*riya'yata* it is spinning, whirling

In the case of *mï'ma* 'rolls over', where necessarily a lateral motion accompanies the turning, we get this phase of the action necessarily extended in *mïma'mata* 'it is rolling along'.

Another type of this general class of phenomena is one which manifests punctually as a shock, jar, or other sudden disturbance necessarily momentary in nature, and is related to a pulsative phenomenon also occurring in the natural world as a rapid succession of such shocks. Here English generally employs two different stems, but Hopi simply

uses the punctual and segmentative of the same stem. An example from inanimate nature is *ti'li* 'it receives a slight jar', *tili'lita* 'it is vibrating' (like an engine, a wagon, an automobile). But Hopi also discerns a great many such phenomena in the animate world, for example:

ti'ri he gives a sudden start

tïri'rïta he is quivering, trembling

wi'wa he trips over something, or is suddenly caught by the legs, like a lassoed horse

wiwa'wata he is stumbling or hobbling along

ŋa'ro his teeth strike on something hard or gritty, e.g. in the food

ŋaro'rota he is chewing forcibly on something hard

hɛ'ro he (or it) gives out a sudden hollow gurgle from within

hɛro'rota he is snoring

Often, again, such verb-forms are applied to rhythmical movements of the body and limbs:

wï''kï he takes a step without moving from place

wïkï'kïta he is doing steps, or dancing in one place

kʷi'la he takes a step forward

kʷila'lata he does a walk forward (not 'is walking forward' which English expression is almost a punctual)

yo''ko he gives one nod of the head

yoko'kota he is nodding

Again, the phenomenon may be one of disturbance at a point in a subtle medium, that is one that would be scientifically classed as gaseous or etheric. Such a medium gives little or no evidence of either motion or extension in space, and the segmentative in these cases denotes only pulsation in time:

rï''pi it gives a flash

rïpi'pita it is sparkling

ʔï'wi it flames up

ʔiwi'wita it is flaming

ʔï'mï it explodes, goes off like a gun

ʔimi'mïta it is thundering

Finally, there is one class of events to which the segmentative is not applied. It is not applied to 'mental', 'emotional', or other 'inner' or 'psychological' experiences. It concerns only the world of external observation.

All this has a wider interest than the mere illustration of an aspect-form. It is an illustration of how language produces an organization of experience. We are inclined to think of language simply as a technique of expression, and not to realize that language first of all is a classifica-

tion and arrangement of the stream of sensory experience which results in a certain world-order, a certain segment of the world that is easily expressible by the type of symbolic means that language employs. In other words, language does in a cruder but also in a broader and more versatile way the same thing that science does. We have just seen how the Hopi language maps out a certain terrain of what might be termed primitive physics. We have observed how, with very thorough consistency and not a little true scientific precision, all sorts of vibratile phenomena in nature are classified by being referred to various elementary types of deformation process. The analysis of a certain field of nature which results is freely extensible, and all-in-all so harmonious with actual physics that such extension could be made with great appropriateness to a multiplicity of phenomena belonging entirely to the modern scientific and technical world—movements of machinery and mechanism, wave processes and vibrations, electrical and chemical phenomena—things that the Hopi have never known or imagined, and for which we ourselves lack definite names. The Hopi actually have a language better equipped to deal with such vibratile phenomena than is our latest scientific terminology. This is simply because their language establishes a general contrast between two types of experience, which contrast corresponds to a contrast that, as our science has discovered, is all-pervading and fundamental in nature. According to the conceptions of modern physics, the contrast of particle and field of vibrations is more fundamental in the world of nature than such contrasts as space and time, or past, present, and future, which are the sort of contrasts that our own language imposes upon us. The Hopi aspect-contrast which we have observed, being obligatory upon their verb forms, practically forces the Hopi to notice and observe vibratory phenomena, and furthermore encourages them to find names for and to classify such phenomena. As a matter of fact the language is extraordinarily rich in terms for vibratory phenomena and for the punctual events to which they are related.

MISCELLANEA

VENETIC INSCRIPTION NO. 169 (CONWAY)

An interesting and valuable discussion of this inscription was given in LANG. 12. 23–34 (1936) by E. F. Claflin; and to this article I refer for all details of bibliography. The text is undisputed: *venna tolar maχetlon*; tho it rests only on the reading of Meyer, confirmed by Pauli, Conway not having had an opportunity to study the document.

For its interpretation I accept: the general belief that *Venna* is a proper name; also Pauli's idea that the following word is a verb, Sommer's identification of the *r* as a personal ending, Claflin's connection of *tolar* with Latin *abs-tulās*, etc. In attempting to advance further I postulate on account of the formalism of Venetic inscriptions: (1) The remainder of the inscription contains a word for 'me'; (2) *Tolar* is not synonymous with *zonasto, zoto*.

The only form attested for 'me' is *meχo*. Since Venetic engravers are frequently inaccurate, it is worth while to mention the possibility that the vowels have been transposed and that *meχ atlon* should have been written. The last word would be a borrowing of ἆϑλον and the inscription would mean 'Venna won me as a prize'—in a horse-race no doubt. The meaning suggested for *tolar* would be in keeping with a meaning of φέρω and of *fero* which *tuli* supplements.

It is better to avoid, if possible, the correction of an inscription. The form *meχo* is attested only at the beginning and end of a sentence. There is no difficulty in assuming that a continuant of IE *me* persisted beside it in less stressed positions. The text is then to be divided *m aχetlon*. The last word I endeavored to connect with ἄεϑλον, but for various reasons it seemed probable that a borrowing of this form would have resulted in **ahetlon*. Since the characters transliterated χ and *h* are much alike I requested Professor Kretschmer to verify the reading. He most kindly informed me that the inscription cannot now be located in the museum. At the same time he pointed out that *aχetlon* corresponds precisely to ἄγεϑλον—a word found only in an inscription (SGDI 1267) from Sillyon in Pamphylia.[1] The meaning is not clear.

[1] For a technical term of the ritual to be attested only in such widely separated places as Pamphylia and Venetia is an indication merely of the fragmentary nature of our sources.

Liddell and Scott render 'sacrificial victims', which seems a bad guess; Bechtel (Gr. Dial 2. 821) cites Meister's 'Festgeräte', but declines to say more than that the word is a derivative of ἀγε-. Professor Kretschmer points out that it is a thing ᾧ τις ἄγει, and suggests a vessel for carrying a libation. Since ἄγειν is used largely with animate objects I should prefer to speak of a halter with which the victim is led to slaughter, comparing ἀγωγεύς 'leading-rein, leash'. The headgear of animals can be richly decorated (cf. Homer, Δ 141–5), and would then be suitable as prizes. I should translate: 'Venna won me, the ἄγεθλον'.

<div style="text-align: right">GEORGE M. BOLLING</div>

SANSKRIT *paṭṭanam*: LATIN *portus*, ETC.

To date, no satisfactory etymology has been suggested for Skt. *paṭṭanam* or *pattanam* 'town, port-town' (quoted chiefly or solely in the latter form by most authorities[1]). Brugmann[2] connected it with the root **pad* in *pi-bdamāna-* 'fest werdend', *pi-bdana* 'fest', and would ascribe to *pattanam* the original meaning of 'Befestigung'. But since the meaning of *pibdamāna-* itself is uncertain[3], this etymology is not wholly satisfactory. Uhlenbeck (Etym.Wb. 154) lists the word as 'unexplained'.

In Böhtlingk und Roth, MacDonell, and Wilson we find *paṭṭanam* (with cerebral *ṭṭ*) listed as a secondary form to *pattanam*, which it is with regard to frequency of occurrence (e.g., in the *Arthaçāstra* of Kauṭilya *paṭṭanam* occurs but once, to the six or seven occurrences of *pattanam*). Its most usual meaning is that of 'port-town'. The word occurs also in Pāli, solely in the form *paṭṭanam*, with the well-attested meaning of 'port, port-town'. (Childers' and Pāli Text Society's dictionaries.)

From the Pāli form *paṭṭanam* and the absence of the word *pattanam* in earlier Sanskrit, it would seem reasonable to accept the assumption made in the Pāli Text Society's dictionary, that *paṭṭanam* is to be regarded as a borrowing from Pāli into later Sanskrit, then frequently modified to *pattanam* without cerebral. The geminated cerebral in Pāli points to the well-known correspondence Skt -*rt*- : Pāli *ṭṭ*, as in *bhartṛ* : Pāli *bhaṭṭa*, *vartate* : Pāli *vaṭṭati*, etc; and would indicate an earlier

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[1] Brugmann; Böhtlingk und Roth, MacDonell, Stchoupak-Nitti-Renou Sanskrit dictionaries; Uhlenbeck, Etym. Wb. der Ai. Spr.

[2] Grundriss 2.1. § 185.

[3] Cf. Hillebrandt, Vedachrestomathie 100, s.v. *pibdamāna-*, and references there cited.

*par-tana-m, from IE *per-teno-m or *por-teno-m. This immediately brings paṭṭanam into connection with the group of words derived from IE *per- 'pass through or across', and which have the general meaning of 'passage, entry, bridge': Av. pərətuš, pəšuš, from *pr̥-tu-; Lat. por- tu-s, either from *pr̥-tu- with zero-grade or from *por-tu- with o-grade, as in Greek por-thmos, 'Überfahrtsort'; and ON. fjǫrðr, possibly also Umbr. per-to-m[4], from IE *per-tu-.

<div align="right">Robert A. Hall, Jr.</div>

PORTUGUESE nosso AND vosso, nós AND vós

The -ss- in Portuguese nosso, vosso (with open o), the possessive ad- jectives of the first and second persons plural, has puzzled many schol- ars in the Romance field. The usual treatment is that which Huber (Altportugies. Elementarbuch §188.3 Anm.) accords the problem. He accepts the hypothesis that these forms are derived from Vg. Latin nostru, vostru. He states that t between s and r was assimilated to s and the r disappeared; this, in spite of the examples which he himself quotes that show that t between s and r maintained itself: astroso, es- tradu, mostrar. To strengthen this explanation, Huber quotes some modern dialectal Spanish words in which -str- became -ss-: mossar < mostrar, maesse < maestre.

It is obvious, however, from the occurrence of an Old Spanish vueso which corresponds exactly to the Port. vǫsso, that both of these words derive from a word that was in use during the period of the unity of Spanish and Portuguese. This eliminates the value of the evidence from modern dialects, for there is no evidence excepting these posses- sive adjectives for the change of -str- to -ss- during the period of unity. Since this change occurs in only two words which in any case were likely to have been treated similarly, it is quite impossible to treat this change as a regular phonetic change when the regular phonetic development attested by all other Portuguese words containing -str- dating from the period of unity was unchanged -str-.

The following history may be suggested for these forms. During the period of the unity of Spanish and Portuguese, by the analogy of the relation of Vg. Latin me : meu, supported in large measure by the rela- tion of Vg. Latin tu : tuu, a similar relation was established in the possessive adjectives of the first and second persons plural, thus nos : *nossu (with double s to indicate that the s was voiceless), vos : *vossu.

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[4] With regard to Umbr. per-tom, cf Osthoff, IF 8. 22; per-tom may possibly not represent IE *per-tu- at all, but may be from *per-i-tom, to the root *i-.

But the *o* of *nós* was originally close, being derived from Lat. *nōs*, and the first *o* of *nosso* is open (cf. Old Sp. *vueso*). This apparent contradiction can be explained on the basis that the vowel of **nossu* and **vossu* was affected by the open vowel of the coexisting forms **nostru* and **vostru*. The proof of the coexistence of these forms lies in the fact that, in Spanish, *nuestro* and *vuestro* (from **nostru*, **vostru*) became the dominant forms, while in Portuguese it was *nosso* and *vosso* that prevailed.

If the preceding is correct, the explanation of the open vowel in modern Port. *nós*, *vós* is clear: it has received its quality from the open vowel of the corresponding possessive adjective.

<div align="right">ISIDORE DYEN</div>

A REPORT ON A LINGUISTIC EXPEDITION TO JAMES AND HUDSON'S BAYS

The following[1] is a summary of the principal results of a linguistic expedition to James and Hudson's Bays in the summer and early fall of 1935, financed by the American Council of Learned Societies: East of Hannah Bay, Cree leaves off and Montagnais-Nascapi begins; as I surmised previously, the so-called Rupert's House Cree and East Main Cree are not Cree at all but Montagnais-Nascapi dialects (see Michelson, Annual Report, Bureau of American Ethnology 28. 247–8; Current Anthropological Literature 1. 290; Explorations and Field-Work of the Smithsonian Institution in 1923, 102; see also Speck, American Anthropologist, N.S., 25. 457[2]). Beginning with Rupert's House, taking in Waswanipi (for Cooper, Atti del XXII Congresso Internationale degli Americanisti 2. 459, proved Waswanipi is a Montagnais-Nascapi dialect, and a [y]-dialect, though Davidson, Indian Notes, Mus. Amer. Ind. 5. 42, said when speaking of them 'who speak a supposedly Cree dialect'), Mistassini, Nichigun, and continuing along the east coasts of James and Hudson's Bays to a little beyond the Great Whale River we have Montagnais-Nascapi dialects in which original [l] is replaced by [y]; the dialects at Fort George and the Great Whale River form a distinct sub-group in which [ē] is entirely or largely replaced by [ā]; the dialects of the Great Whale River and Fort George touch that of Fort Chimo in which original [l] is replaced by [n] (based on unpublished material of Turner), thus agreeing with the dialects of Davis Inlet and the Northwest River; Mistassini touches the dialect of Lake St. John in

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[1] Printed by courtesy of the Smithsonian Institution.
[2] See Michelson, ibid. 26. 295.

which original [l] is maintained. The exact position of Cree at the Albany River is a little uncertain. Documentary evidence tends to show that it is a Cree dialect in which original [l] is replaced by [n] (see Horden, A grammar of the Cree Language 3 and Skinner, Notes on the Eastern Cree and Northern Saulteaux, Anthrop. Papers Am. Mus. N. H. 9.11) but owing to the present-day diverging usage of Anglicans and Catholics it is impossible to be sure without going there in person. The few Albany Cree I met elsewhere disagreed, some claiming the language was the same as Moose Cree. However, the dialects at Atta-wapiskat, Wenusk (both unreported upon till now), Severn, and York Factory on the west sides of James and Hudson's Bays are Cree dialects in which original [l] is replaced by [n], thus agreeing with the dialect at The Pas, Manitoba (on which see Bloomfield, XXII Congresso Inter-nazionale degli Americanisti 2.427). Kesagami Cree, south of Moose Cree is practically extinct, though it is still possible to obtain isolated words. It is an [r]-dialect, that is, one in which original [l] is replaced by [r]. Owing to lack of data it is impossible to know whether Kesa-gami Cree is genealogically to be connected with Tête de Boule, also a Cree [r]-dialect (so Cooper, Festschrift P. W. Schmidt 206, and Michel-son, American Anthropologist, N.S., 35. 396 as opposed to Davidson, AA, N.S. 30. 20–1) or whether the change from original [l] to [r] is merely a parallel one. Similarly there are insufficient data to know whether the Cree dialect at Isle à la Crosse, also an [r]-dialect, genealogically be-longs with Tête de Boule or not. The northern and eastern boundaries of Plains Cree, in which original [l] is replaced by [y], are at present nearly unknown; according to Bloomfield, loc. cit., it extends (at least) as far north as the city of Prince Albert. It is, of course, well-known that Cree of the English River is a dialect in which original [l] is replaced by [th]; the exact boundaries are unknown. The Ojibwa spoken at the Albany River differs from that spoken in Wisconsin, Minnesota, etc. It may be noted that [ä] is replaced by [ē]. There are no data available on other Ojibwa bands in this northern country; so it is useless to specu-late on the interrelations. The Eskimo spoken at the Great Whale River is distinct from that at Fort Chimo and that spoken on the Belcher Islands. The Eskimo spoken in south-eastern James Bay region is said to be the same as that spoken on the Belcher Islands.

TRUMAN MICHELSON

BOOK REVIEWS

Linguistic Analysis of Mathematics. By ARTHUR F. BENTLEY. Bloomington; The Principia Press, 1932.

Behavior, Knowledge, Fact. By ARTHUR F. BENTLEY. Bloomington: The Principia Press, 1935.

These books deal with several topics which your reviewer believes can be usefully studied in the light of linguistics. One of the books has the word 'linguistic' in its title, and the words 'language' and 'linguistic' occur very frequently on the pages of both. Moreover, they contain many sentences which, torn out of context, would hold the same promise, such as LA 38: 'Every exact analysis, in, of, or by means of language, rests in preliminary provisional dissection and organization of linguistic materials.' 59: '. . . we proceed to examine linguistic phenomena in full play.' 63: 'We proceed, thus, under a full linguistic-semantic inspection.' BKF 9: 'A science of language exists, of course, just as much as does a science of psychology', and so on.

This appearance, however, is deceptive. The word 'language', with its derivative 'linguistic', in these books does not mean that which linguists and many other people call 'language'. What it does mean, your reviewer, after careful reading, is unable to say. LA 32 we read: 'Language may be studied in terms of printed page, writing hand, reading eye, hearing ear or speaking voice.' In this statement any linguist will recognize a widespread popular error. The next sentence, however, says, 'In such specialized studies we have here no technical interest.' Then, 33: 'Language is a functional of Experience, of Knowledge and of Fact: and Experience, Knowledge and Fact are functionals of it, and of another', and 35: 'Language subdivides most generally into (a) Inchoate Implication. (b) Words-common. (c) Terms. (d) Symbols.' These subdivisions are then explained. Then again, BKF 131: 'Language, so viewed, is no such limited abstraction as is the "language" of the grammarian or philologist . . .' 145: 'We proceed now to the linguistic aspect', but again the linguistic reader's hopes are shattered, for the next sentence reads: 'Aristotle studied language and gave us laws of reasoning.'

Now and then, to be sure (as in LA 32, quoted above), Bentley does

speak of 'language' as linguists use the term, and when he does so, it is not in a way to inspire confidence.   Thus, LA 18: 'the "Essai de sémantique" of Michel Bréal, who concentrated attention upon evolutions of linguistic meaning as contrasted with the prevalent philologies specializing upon linguistic form', where, aside from the false emphasis, the plural of 'prevalent philologies' is puzzling.   The most astounding passage of this sort is BKF 9: 'Neither the "abstraction" set up for language by the professional philologist, nor a close specialization for "behavior" in some narrowly "mechanistic" or "mental" form, will show language and science themselves in wide spatial and durational spreads.'   This simply does not make sense; one can infer only that the whole body of nineteenth-century linguistic work, which deals almost exclusively with the 'spatial and durational spreads' of language, does not exist for Bentley.

A large section (BKF 229–82), finally, develops the foundations of linguistics without benefit of the generations of work that have been devoted to this subject—and the result is just about what one might expect of such an attempt.

A writer, of course, has the privilege of defining and using words in any way he sees fit.   If we are right in assuming that Bentley wishes to re-define (in the vaguest of terms, to be sure), the words 'language' and 'linguistic', then some of his statements about 'language', 'philology', and the like are lapses from his new definition into the current meaning of the terms, and they are unfortunate lapses, because he is evidently innocent of linguistics.

Confusion is all the more likely since some students believe that the topics with which Bentley is dealing receive a great deal of light from the study of language.   The only psychologist, so far as I know, who has appreciated the working of language in human behavior, is A. P. Weiss.   Occasionally Bentley writes as though he followed Weiss; thus, BKF 149: 'Language, taken in this way, absorbs very largely— in the end, we may even find, entirely—the meanings or references which are commonly ascribed to "thought" and assumed to inhabit the region at the far side of the "physical-to-psychical" jump.'   However, it always turns out that Bentley does not mean 'language' in the linguist's sense of the word.   In fact, Bentley's summary of Weiss's system (BKF 58) shows that Bentley fails to understand precisely this aspect of Weiss's work.   Bentley does not grasp the linguistic basis of Weiss's 'biosocial' classification ('social factor', 'response', etc.), or the central position of these terms in Weiss's system.   Instead, Bentley

views these terms as a discordant element arising in spite of Weiss's strictly physical hypothesis and threatening to break it down; thus, 81: 'We find Weiss, in his movement-space, overwhelmed by something called "social".'

Perhaps the case of Bentley gives us the explanation for the fact that many keen students fail to understand Weiss's meticulously clear exposition. When Weiss speaks of 'language' he means exactly what he says, the language which is studied by linguists, the noise you make with your face. The linguistically untrained reader is so thoroughly accustomed to regarding (or rather, disregarding) language as some sort of mere subsidiary noise for the 'expression of ideas', that he fails to accept Weiss's words in their plain meaning. It is as though a citizen, going to court for some minor business, were greeted by the judge with the words, 'Good morning, I sentence you to death'. Under all the presuppositions of our place and time, the citizen would fail to accept these words until he felt the noose round his neck. One cannot imagine a clearer statement than Weiss's; the difficulty is only that many readers cannot take his words at their face value because these words utterly contradict the readers' presuppositions, and this is especially the case of readers who are not accustomed to discourse about language.

We may go a step farther. Weiss believed, and this reviewer believes, that the topics which Bentley is discussing cannot be elucidated without linguistics. In simple cases even the reviewer can see how the lack of linguistics hampers Bentley's discussion of cognate matters. Thus, BKF 59: 'We need spend no time on Weiss' electron-proton theory, which has merely the status of a counterblast to the "mentalists"; it is just one dogma against another. As an unused hypothesis it is psychologically sterile. . . .' Yet Weiss plainly says (Theoretical Basis[2] 16 footnote 1929) that this is not his hypothesis, but the hypothesis of physics at the time of writing; that physics is sure to change its basic hypotheses; that the only important point for us is that we discuss human behavior in physical terms. The same conclusion has since then been reached, independently and along the route of logical analysis, by H. Neurath and R. Carnap (see, for instance, the latter's Unity of Science, London, 1934). Their rule of 'physicalism' says that scientific discourse makes serse only when it speaks in physical terms. Be this as it may, when Weiss tells us from where he is taking the undefined terms of his system, he is neither enouncing a dogma nor giving us an unused hypothesis. Bentley's lapse here seems to result from a failure to grasp the linguistic conditions of scientific discourse.

Again, LF 16: 'A fully clarified postulation is, however, not attained until all alternative postulates are explored. Illustrative of this is the Euclidean parallel axiom.' Bentley's demand here is impossible, since one could go on inventing ('alternative') postulates for ever. That this demand rests upon a misunderstanding of the linguistic situation appears in the explanatory footnote: 'Until alternative axioms were brought to light and studied a century ago, there was a defect in Euclidean geometry yet waiting to be revealed. This defect was realistic, under the distinctions of the text.' Actually, the question, open until a century ago, about Euclid's parallel axiom, was a question of independence: was Euclid right in making this statement an axiom, or could the statement be proved as a theorem on the basis of Euclid's other axioms? When it was shown that consistent geometries could be developed without this statement, it appeared that Euclid was right, that, emphatically, this was not a defect in the Euclidean geometry. It is Bentley who here makes a 'realistic' lapse, and this lapse is due to Bentley's non-linguistic approach.

Similarly, Bentley's sketch of a definition of the mathematical term 'group' (LA 81) omits the features that are most important for the discussion in hand, and his claim that mathematicians speak of a group alternatively as a mathematical operation upon the objects or as itself a mathematical thing 'in which latter case the original objects appear as operations within it' contains several misapprehensions of a kind that will scarcely be made by a linguistically trained student, even if his mathematics be as poor as the reviewer's.

The chapter on the denumerability of decimals (LF 181) is a good illustration of non-linguistic attempts at discussing the foundations of mathematics. Since mathematics is a form of speech, information about its necessary modicum of undefined terms can be obtained only by linguistic considerations. Beyond this, the mathematician may occasionally, by way of luxury, ask for a report upon the linguistic character of a defined term, such as 'infinity'. The linguist's contribution will be rather trivial, but may give comfort. Thus, it requires no great linguistic insight to show that the mathematician's 'denumerable infinity' and 'non-denumerable infinity' name two different types of convention as to the use of words. The thing can be proved within mathematics, without discussion of the linguistic background. However, instead of supplying the linguistic scholium, Bentley confuses what the mathematicians have not confused.

A linguist will naturally object to a treatise which claims to deal

with 'language' but is at no point in touch with the methods and results of linguistic science, and he will naturally question whether a system of psychology that is so constructed can add anything to what we know.

LEONARD BLOOMFIELD

Études indo-européennes. 1. Pp. 14 + 294. By JERZY KURYLOWICZ. (Polska Akademja Umiejętności Prace Komisji Językowej, Nr. 21). Cracow, 1935.

Kurylowicz gives us in highly condensed form daring and extremely ingenious suggestions for rewriting a good part of Indo-European comparative grammar. Chapter 1 (Les occlusives labiovélaires) undertakes to show that primitive Indo-European had only palatals and pure velars, but that in the centum-languages inherited *ke* (pure velar + *e*) came, by a dissimilative process, to be identical with inherited *k̂we* and *kwe* (palatal or pure velar + *w* + *e*). At least this is the way the author puts it; but it appears from the second paragraph on page 22 that he thinks of the original pure velars as involving lip rounding. If then we translate his terminology into that employed by other linguists, he assumes for primitive Indo-European palatals and labiovelars, the latter series losing the labialization in the *satem*-languages and before consonants, *u, o,* and *a* in the *centum*-languages—a thesis that seems considerably less radical.

Chapter 2 (Sur les éléments consonantiques disparus en indoeuropéen) sums up, supplements, and develops the author's doctrine of the Indo-European laryngeal consonants. In LANG. 6.149–58 (1930) I discussed certain of Kurylowicz's views on this matter and found what seemed to me fatal weaknesses. The theory is now greatly improved, and in the meantime Sapir has, by word of mouth, called my attention to certain evidence that goes far toward establishing it. The matter is of such capital importance that I shall try to put Kurylowicz's most important conclusions into readily understandable form by eliminating his algebraic symbols and incorporating certain additions and corrections (from 254 f.) with the original text.

Kurylowicz designates the laryngeals as $ə_1$, $ə_2$, $ə_3$, and $ə_4$, for which I shall substitute respectively (following Sapir) '(glottal stop with palatal color; cf. Egyptian *i*), *x* (voiceless velar spirant), γ (voiced velar spirant), and ʾ (glottal stop with velar color; cf. Egyptian ʾ). It must be clearly understood (1) that Kurylowicz nowhere implies these identifications, and (2) that Sapir and I do not assert their complete accuracy.

The chief advantage in assuming them is that most linguists find it necessary 'to make a noise', as Bloomfield once expressed it in conversation.

Kurylowicz's revised doctrine may, then, be summarized and modified as follows:

1. Every original long vowel (i.e. every long vowel not due to later lengthening or contraction) comes from compensatory lengthening upon loss of a laryngeal consonant before another consonant (before a consonant $e' > \bar{e}$, $ex > \bar{a}$, $e\gamma > \bar{o}$, $e'. > \bar{a}$).[1] Furthermore o (by qualitative ablaut from e) yields $\bar{o}$ upon loss of ' before another consonant. All laryngeal consonants between a vowel and a consonant are lost except that x appears in Hittite as h. Hence we may illustrate the five phonetic laws here stated as follows (without attempting a complete reconstruction of the several words): *$dhe'ti$ > Skt. $dadh\bar{a}ti$, Gk. τίϑησι 'he places'; *$pexwn$- > Hittite $pahhwenas$ (gen.), Goth. $f\bar{o}ns$ 'fire'; *$de\gamma ti$ > Skt. $dad\bar{a}ti$, Gk. δίδωσι 'he gives'; *$(s)te'.mi$ > Hittite $tittami$[2], Gk. ἵστημι 'I cause to stand'.

2. When a laryngeal consonant stands between vowels it is lost without compensatory lengthening, except that in Hittite x between vowels becomes hh. Examples are *$dhe'iy\bar{e}m$ > Skt. $dhey\bar{a}m$, Gk. ϑείην (aor. opt.); $xeuxos$ > Hittite $huhhas$, Lat. $avus$ 'grandfather'; *$de\gamma iy\bar{e}m$ > Skt. $dey\bar{a}m$, Gk. δοίην. I shall try to show in the next number of LANGUAGE that ' between vowels remains in Hittite, and is written h; e.g. *$se'ur$ > Hittite $sehur$ 'urine': $se'mn$ > Lat. $s\bar{e}men$ 'seed'.

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[1] Kurylowicz holds that the origin of the lengthened grade vowels was subsequent to the loss of laryngeals with compensatory lengthening. I do not know how he reconciles this with the retention of at least one laryngeal in Hittite. I may add that Sapir will presently demonstrate the retention of laryngeals in other languages also.

In Symboli Grammatici in Honorem Joannis Rozwadowski 96 Kurylowicz wrote: 'Posons donc de façon toute mécanique $e + \partial_1 = \bar{e}$; $e + \partial_2 = \bar{a}$.' I inferred that the author assumed lengthening of a preceding vowel upon loss of ∂, regardless of the nature of the following sound, and I so quoted him in LANG. 7.119. In 1932 Kurylowicz told me that he assumed lengthening upon loss of ∂ only before a consonant, and he referred me to a passage in an article of his in Rocznik Orjentalistyczny 4.214 f., where a distinction is implied between the position before a vowel and before a consonant. Kurylowicz quite properly corrects this error of mine in the work under review (12 f.). I have just now learned from BSL 36.27 footnote 1 that he has been expecting an apology from me!

[2] Bo. 2026 b, cf. KUB 1 p. 8: ti-it-ta-mi; KBo. 4.8, 2.6: ti-it-ta-nu-un; KUB 14.16.4.17: ti-[it]-ta-nu-un. Others may prefer to read $titta<nu>mi$, and $titta<nu>nun$.

3. A laryngeal consonant between two consonants is lost without trace except in Greek and Armenian, where a secondary vowel (43, voyelle secondaire) is developed. This is the source of many of the 'prothetic' vowels of Greek. Examples are *dedɣté > Skt. dattá, Gk. δίδοτε 'give ye'; *xnérm > Skt. náram, Gk. ἀνέρα (acc.) 'man'. It seems clear that this conclusion must be modified to the extent of assuming that the vowel of Greek and Armenian is not a secondary development but rather a retention of a minimal vowel (schwa secundum); Skt. dattá < *-dɣté, Gk. δίδοτε < *-dʊɣte. If so we can easily account for Indo-Iranian i = European a in the traditional heavy bases in the same way. As far as I can see this is the only way to connect OIsl. saurr 'male semen' with Hittite sehur 'urine' and Lat. sēmen 'seed'; the OIsl. word must come from *sʊ'ur-. Kurylowicz, however, accounts for this schwa Indogermanicum in an entirely different way, namely:

4. A laryngeal consonant between a consonant and a vowel is lost, changing the quality of the vowel in the same way as that of a preceding vowel. In Indo-Iranian it converts a preceding voiceless stop into a voiceless aspirate. A reduced vowel after a lost laryngeal consonant appears as Indo-Iranian i, Greek a, ϵ, o, general European a. The development of the voiceless aspirates is surely a more complicated process than Kurylowicz realizes. He calls attention to Gk. -ϑα beside Skt. -tha in the 2 sg. perfect; since Hittite shows -ta in the 2 sg. preterit of the hi-conjugation, we must assume here original -t'e, and conclude that the glottal stop with velar color combined with t to form a voiceless aspirate in Greek as well as in Indo-Iranian, although txe yielded Gk. τα beside Skt. tha. In ascribing schwa Indogermanicum to original 'e, xe, ɣe, or 'e, Kurylowicz has in mind such pairs as Gk. ἐκτός : σχετός beside ἔχω < *seĝhe-; with ἐκτός he would compare Skt. -ttas 'given' and with σχετός Skt. hitás, Gk. ϑετός 'placed'.

5. Every Indo-European word beginning with a vowel has lost an initial laryngeal consonant; e.g. *'esti > Hittite eszi, Skt. asti, Gk. ἐστι 'he is'; xanti > Hittite hanti 'in front, separately', Skt. anti 'opposite, before', Gk. ἀντί 'opposite'; *ɣekw- > Gk. ὄψ 'eye'; 'epo > Hittite appa 'afterwards, again', Gk. ἀπό 'from'.

On the whole this formulation of the hypothesis of Indo-European laryngeals is very attractive. I have little doubt that it will be further modified by a considerable body of evidence that Kurylowicz does not take into consideration, and that it will in the end be generally accepted.

Chapter 3 (Les changements vocalique et leurs chronologie) contains the remainder of the author's theory of ablaut. The first and most im-

portant section gives an elaborate chronology of the weakening, loss, and lengthening of vowels in pre-Indo-European times. The first weakening, characterized by total loss of certain unaccented vowels, is subdivided into five chronological stages, as follows: (Ia) a medial vowel following the accent was lost (*'enék̂e > *'énk̂e); (Ib) other vowels immediately following the accent were lost (*'enék̂e > *'enék̂, *séĝhe > *séĝh); (II) further vowel losses produced initial and final consonant groups and interior groups of three consonants. (IIa- *'enék̂ > 'nek̂, *seĝhé > *sĝhe; IIb —*'enk̂e-té > 'enk̂té; IIc—*'énk̂e > *'enk̂.) The second weakening was characterized by the reduction instead of loss of unaccented vowels; it is likewise divided into several successive stages. Vowel lengthening belongs somewhere in the period of the second weakening, and is due to the loss of a vowel that had been reintroduced by analogy. Needless to say, the evidence for so complicated a theory of prehistoric chronology is indirect and largely subjective. One may grant at once that the theory of Hirt and Güntert is unsatisfactory in that it provides no workable explanation for the existence of reduced grade and zero grade side by side; but a far simpler explanation than the one here suggested can certainly be found.[3]

Chapters 4 (Remarques sur la flexion nominale) and 5 (Notes de dérivation nominale) consist largely of an application of the author's system of ablaut to certain details of case formation and noun formation. In the nature of the case the theories here presented cannot be any more secure than the foundation upon which they rest. Since I am unable to accept the results of Chapter 3, it is not surprising that I am in general unconvinced by the morphological discussions of the succeeding chapters.

The book is closed by thirteen pages of addenda and corrigenda and very full indices.

E. H. STURTEVANT

Beiträge zur Geschichte der Romanismen: I Chronologische Phonetik des Französischen bis zum Ende des 8. Jahrhunderts. By ELISE RICHTER. Halle: Niemeyer, 1934.

The chief points in Frau Richter's program are: (1) To study the development of the phonological phenomena concerned with the evolution of the French language, replacing hitherto current systems by a purely chronological approach; (2) To include all those phenomena necessary

[3] Cf. Sturtevant, LANG. 11.184 fn. 15.

for a complete picture, setting as a *terminus ad quem* the eighth century, the period when French begins, and as *terminus a quo* the oldest of Latin, *ab urbe condita* (31, 34, 42, 55), going back even to Indo-European, in rare cases (41).

There is apparently no blame to be attached to either of these points. As to (1), in so far as pre-conceived logical analysis is displaced by chronological description, there is great progress since the days of the eighteenth century 'raisonneurs'. As to (2), though the *terminus a quo* seems to be arbitrarily set and although one's curiosity remains unsatisfied with reference to some pre-Latin phenomena of possible interest to Romance, temptation to go always further back is wisely resisted.

This laudably ambitious project is fraught with difficulties. Some could have been avoided, doubtless: the objective chronology of a phoneme should ideally do away with any provocative discussion as to the unity of Latin or the necessity of defining Vulgar Latin. Other difficulties, we fear, are inherent in the subject. The author realized that she 'musz und wird die Kritik herausfordern', that it was a matter of 'nahezu unüberwindlichen Schwierigkeiten' to restore the 'shattered mosaic' and give it living expression (3), that it was next to impossible to dissociate the appearance of a phenomenon at a given point and the date thereof from the determination of its spread, i.e., the social could scarcely be dissociated from the physiological history in point of time (30): 'die langsame Entwicklung der Lautungsänderung ist nicht zu verwechseln mit der langsamen Verbreitung der Sprechweise.' Her perspicacity did not, however, keep her from inevitable pitfalls. She notes that the spoken form antedates the written. Granted that this be true in a time when learned sources were few in number and geographically restricted, it is perilous to decide just how far back of its first attestation a given form was first produced. Yet this is what the author attempts. Thus (181) nasalization is dated further back than many historical grammarians are willing to grant: 'Die Nasalierung von *e* erfolgt erst nach dem Ende dieser Periode (that of *a* being within it), die von *o* beginnt vor dem 11., die von *i* und *u* nicht vor dem 12–13 Jahrh.' Brunot-Bruneau, Gr. Hist., state that *a* and *e* nasalize in the 'époque prélittéraire', *o* in the 12th century, while *i* and *u* do so many centuries later (116 ff.). On the contrary, she proves her point as to the early vocalization of *l* against Bourciez, Précis 246, who would place it in the 10th century, certainly much too late. The author could have avoided criticism by limiting herself more to actual dates of occurrence, letting the reader allow his own margin between these dates

and those of probable origin. This procedure might have made it more difficult for the starred forms, which, alas, are heavily exploited. As parallels to the attested forms, they are useful, e.g., §36. In §82, all the forms are posited. Approaching the 'twilight zone' (5th and 6th centuries) one finds the pages loaded with asterisks (126, 138, 144 et al.), eloquent evidence of the rarity of texts and the 'Kargheit der Belege' (241). The story would be more graphic if those texts that we have were more intensively discussed as to date. What, in fact, is that of the Appendix Probi? Does it fall into the period between the 3rd and 6th centuries or is it earlier?

The limitations an author chooses to set for his subject can rarely be blamed. In this case, the absence of systematic semantic treatment allows some obscurity. Why are *communicare, delicato, aboculo* (172) more in the 'Alltagsprache', i.e., liable to syncope, than *petrica* (a starred form, be it noted)? There seems to be no determined policy; a fortunate situation, since it admitted the excellent semantic note on OF *nice* (193). Morphology likewise enters secondarily. It is to be hoped that the too succinct allusions to 'Systemzwang' (227 for example) or to an unexplained 'Kurzform' will some day be elucidated, as we may hope from the fact that we are here considering volume I of a prospective series.

The disposition within the various sections allows, in fact, but little reference to morphology or semantics. The upper half of the paragraph has the words arranged under their phonetic peculiarities. The lower portion is devoted to discussion of these items, section A being devoted to the physiological processes involved in their evolution up to that point, and section B labelled 'die historischen Feststellungen'. Footnotes appear in a separate paragraph. Here, too, the province of each division is hard to delimit; historical data may be used (e.g., 208) to prove a phonological point in B with the addition of the author's own comment, so that the content is not always 'historisch' nor, necessarily, a 'Feststellung'. There are often sections and sub-sections, complex and ponderous, even for a scientific work not intended for elementary students. The index itself is by no means simple. In looking up *aboculo*, for instance, one is referred to §108, where the word is not to be found; one discovers that the 'Wörterverzeichnis' contains those items 'die für die Wortgestaltung in Betracht kommen, auch die, in denen das Wort selbst nicht genannt ist'. Why could they not have been included in the text? · The present reviewer is at loss to see why §49 treats -*i*- > -*j*-, §50 cons. + *i* + vowel > cons. + *j* + vowel, §51 *n* + *f*, *b* or *v* > *m*, §52 *s* + cons., etc., etc., in that order. Even granted

that a chronological sequence is being attempted, the arrangement does not seem the inevitable one.

These are general statements. To go into complete detail in a book of this comprehensive nature would be to undertake a review of Romance phonology, with many an excursus into related branches. Nor is there any desire either to attack or defend the observations of previous reviewers. Yet there are some details that require treatment:

7. Why should the substratum theory not be brought up again? Richter seems to imply its working in the case of local differences in stress accents, seemingly due to racial speech habits.

13. 'In gegebenen Fällen sieht der Chronist von der Etymologie ganz ab und behandelt den Sprachstoff wie der naive Sprecher der Zeit, um die es sich handelt.' Does the etymologist forget the pronunciation of the speaker? Can the 'Chronist' detach his thought from the periods preceding or following that of his speaker and place himself in the exact environment of that speaker? Is the etymologist not, sporadically, a 'Chronist'?

14–15: Richter states that, following the Carolingian period, French changed 'immer langsamer und wird schlieszlich fast stabil' despite an exceedingly agitated political history. Only since the French Revolution do we observe 'stärkere sprachliche Bewegung, erst seit dem letzten Drittel des 19. Jahrh. deutliche phonetische Umgestaltungen.' It would have been clearer to say what these 'Umgestaltungen' were and whether we can accord to them more importance than to those phonological and morphological traits which separate OF from Middle French.

26. What is the explanation of OF *chanut*, where the initial vowel does not, as in *catedra*, enter into hiatus by the fall of the intervocalic consonant?

A great deal of information, bibliographic and otherwise, is hidden away in odd corners. A complete Index Rerum would have been advantageous. Whatever be the faults of this book, it is a great deal more than an example of great industry. It is a repertory of factual information that no romanist can afford to ignore and it embodies, at the same time an idea, not to say an ideal, bound to be fruitful.

A. H. SCHUTZ

A Manual of Russian Pronunciation. Pp. 124. By S. C. BOYANUS. London: Sidgwick and Jackson, 1935.

Boyanus has given us a brief and competent guide to Russian pronunciation which is on the whole very well arranged and clearly worded;

an excellent format adds to the usability of the book. The necessary minimum of general phonetics is briefly given; the sounds are described and further illustrated by means of mouth diagrams; there is a chapter on intonation (with tonetic transcriptions) and a chapter on length, stress, and rhythm; there are numerous examples and pronunciation exercises; there are rules for reading Russian orthography (by Prof. B. A. Larin). The treatment is based primarily on the author's own pronunciation, which differs in some respects from that recorded by Trofimov and Jones in The Pronunciation of Russian (Cambridge: University Press, 1923). The transcription employed is fairly 'narrow', but the reader is for the most part advised of the phonemic classification of the sounds.

In keeping with the general output of the London school, the description of sounds is a model of comprehensive accuracy and clarity. By contrast, rules of occurrence of sounds tend to be unnecessarily complicated and sometimes incomplete. Thus, we are told (36–7) that ə occurs (1) in final closed syllables, (2) in pre-pretonic syllables, (3) in post-tonic positions, (4) in final open syllables. It is obvious that rules 1 and 4 are only special cases of rule 3, and that they are only two of the various special cases that one might mention if it is necessary to mention special cases. A much more essential rule is not given (but can be arrived at from a note [36], taken together with information [83] given elsewhere), namely that ə occurs in pretonic position after ʃ ʒ c[1].

To mention another rule that is not fully adequate, it is said (29) that 'ɛ is always followed by a hard consonant'. But ɛ also occurs in final position followed by no consonant, e.g. fsjɛ 'all'.

In order to give effective rules of occurrence, it is necessary to find the determining feature or features and to state the rules accordingly. In this way, the distribution of hard and soft variants of the vowel phonemes under the stress can be simply stated as follows: i has the value [i] after hard consonants, otherwise the value [i]; e has the value [e] before soft consonants, otherwise [ɛ]; a o u have the values [æ ö ü] between soft consonants, otherwise [a o u]. Soft consonants include p, b, f, v, m, t, d, s, z, n, l, r, j č ǰ (of limited occurrence), also ʃ when it precedes č, ʒ when it precedes ǰ, and, before front vowels (e i), k g x γ. The hard consonants include p b f v m t d s z n l r ʃ (except before č) ʒ(except before ǰ) c and, before back vowels (a o u), k g x γ. The gutturals and ʃ ʒ are peculiar in being soft in some positions, hard in others. Otherwise, the consonants differentiate soft and hard phonemes in pairs

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[1] For the combinations in one type of ts, tʃ, dʒ, will be used c, č, ǰ.

(p pⱼ, m mⱼ, etc.) except for j č ǰ c and c. The descriptions and diagrams suggest the possibility that č and c may constitute another soft-hard pair; this is of course not true etymologically, but it may be that the two sounds have come to stand in this relation phonetically.

In the treatment of stress, there is no mention made of a middle or secondary stress, but it is necessary to recognize such a stress phoneme in context. Some internal evidence on this point is seen in such sentences as: at'krojt‚ɪ dⱼvⱼerⱼ 'open the door' (75), ɛtə jɪʃčo va'pros 'there's still some question about it' (69), ana u'jɛxələ 'za gərət 'she has gone into the country' (47). Now, as we are told elsewhere in the book, e ɛ and o occur only under the stress, and a occurs either under the stress or in the syllable just preceding. These rules are regular if the middle stress is recognized; thus: at'krojt‚ɪ ‚dⱼvⱼerⱼ, ‚ɛtə jɪˌʃčo va'pros, a‚na u'jɛxələ 'za gərət.

There is no statement of syllabic theory. But a proper enunciatory grouping of phonemes is necessary for the attainment of a true pronunciation, and Russian syllabic theory is sufficiently different from that of English to make a statement on this matter of real value to the English student of Russian. Furthermore, a knowledge of syllabic theory would make the rules of assimilatory voicing and unvoicing more comprehensible. Some indication of syllabic division is given indirectly by the placing of the stress marks before the accented syllable, e.g. ka'gda showing the syllable division ka-'gda.

Boyanus makes inaccurate use of 'essential' and 'non-essential' in the following: 'narrow transcription . . ., i.e. one which employs more symbols than there are essential sounds' (1); 'some of the above symbols, e.g. ɪ, æ, ʌ, represent vowels occurring in Russian as non-essential sounds' (1 ftn). A non-essential sound would seem to mean one that can be omitted without making any difference, but it is obvious that the given sounds cannot be omitted in speaking Russian. The trained scientist can guess what is meant ('one which employs more symbols than there are phonemes'; 'vowels occurring other than as "principal" members of phonemes'[2]), but the usage must be unclear and probably confusing to the uninitiated.

<div align="right">Morris Swadesh</div>

[2] I am personally somewhat skeptical about the value of selecting any given variant as the 'principal' member of the phoneme. Nor has a satisfactory basis for such a selection as yet been offered. According to Jones. An Outline of English Phonetics[3] 49 (1932), 'The most frequent sound of a phoneme is called the Principal Member'. By this definition, [ə] and not [a] is the principal member of the a phoneme of Russian.

NOTES AND PERSONALIA

THE THIRTEENTH ANNUAL MEETING OF THE LINGUISTIC SOCIETY OF AMERICA will be held December 28–30, 1936, at Chicago, jointly with the American Philological Association and the Archaeological Institute of America. Sessions will be held at the Hotel Morrison, which will be the headquarters of the three organizations.

THE LINGUISTIC SOCIETY OF AMERICA AND THE UNIVERSITY OF MICHIGAN will jointly conduct, as a part of the University of Michigan Summer Session of 1936, a Linguistic Institute at Ann Arbor from June 29 to August 21. Apart from the local faculty members, Professor Edgar H. Sturtevant of Yale University, Professor Hayward Keniston of the University of Chicago, and Professor Hans Kurath of Brown University will conduct courses. Professors C. D. Buck and Leonard Bloomfield of the University of Chicago, Professor G. M. Bolling of Ohio State University, and Professor R. G. Kent of the University of Pennsylvania, have been invited to Ann Arbor for special public lectures. Full information can be secured upon application to the Director of the Summer School, University of Michigan, Ann Arbor, Mich.

PROF. DR. OTTO JESPERSEN, Honorary Member of the LINGUISTIC SOCIETY OF AMERICA, will be the Presiding Officer of the Fourth International Congress of Linguists, to be held at Copenhagen August 27 to September 1, 1936. Prof. Joshua Whatmough of Harvard University and Prof. W. Freeman Twaddell of the University of Wisconsin have been appointed official delegates to represent the LINGUISTIC SOCIETY at this Congress.

THE HONORARY MEMBERS elected at the New York meeting have accepted election. The following extracts come from their letters:

Riga, Latvia, 19–1–36: Der Linguistic Society of America danke ich von ganzem Herzen für die mir erwiesene hohe Ehre, auf die ich gar nicht rechnen durfte und die ich daher um so höher einschätze. Die Zugehörigkeit zu den Ehrenmitgliedern der Linguistic Society of America verpflichtet freilich zu hohen Leistungen in der Wissenschaft; da aber meine Wahl nicht ohne Nachsicht gegen meine bisherige Tätigkeit stattfinden konnte, so hoffe ich, dass dieselbe Nachsicht mir gegenüber

auch nächstens geübt werden wird. An gutem Willen, der Wissenschaft nach bestem Wissen und Gewissen zu dienen, soll es mir jedenfalls nicht fehlen, solange ich noch am Leben und gesund sein werde.—J. Endzelin.

University College, Gower Street, London, W.C.1, 10 Jan. 1936: I feel greatly honoured at having been elected an Honorary Member of the Linguistic Society of America. I accept the distinction with very great pleasure, and beg to offer to your Executive Committee and to your whole Society my sincere thanks for conferring it on me. I send all good wishes for the prosperity of your Society and the excellent work it is doing.—Daniel Jones.

MILMAN PARRY, Assistant Professor of Greek and Latin in Harvard University, and a member of the Linguistic Society of America since 1927, died in Los Angeles, California, on December 3, 1935.

He was born in San Francisco on June 20, 1902. He was graduated from the University of California with the degree of A.B. in 1922, and of A.M. in 1923. From 1924 to 1928 he was in Paris, where he studied under Paul Mazon, and gained the coveted degree of Docteur-es-Lettres. After a year at Drake University he was called to Harvard, where he was Instructor in Classics until 1931, and then Assistant Professor of Greek and Latin, until the time of his death.

His chief interest was in the Homeric poems, especially the repeated formulas, which he held to be proofs that the Iliad and the Odyssey were orally composed. In general his work was convincing; its one weakness, perhaps, was his handling of the linguistic problem. From Homer, however, he had passed to the study of a still living tradition of oral poetry, namely that of the Slavonic-speaking people of Jugoslavia, where he spent the year 1924–5 collecting a large number of songs, still orally transmitted, with the object of studying their transmission and hence, by analogy, of throwing light upon the methods of the Homeric composer and singer. His premature death has inflicted a great loss upon the study of Homer in America.

J. WHATMOUGH

WILLIS PATTEN WOODMAN, a Foundation Member of the Linguistic Society of America, died on October 9, 1935, in Geneva, New York, after a brief illness.

He was born at Hyde Park, Mass., on December 11, 1871, and received the degree of B.A., with honors in Classics, from Harvard College in 1895, and the M.A. in 1896. During the next two years he

taught at Pembroke Academy, Pembroke, N. H., and then spent a year at the American School of Classical Studies in Rome. In 1902 he received from Harvard the degree of Ph.D. in Classical Philology, and became Instructor in Greek at Princeton University. After a year as classical master at the Morristown School in Morristown, N. J., he came to Hobart College, where in 1907 he was appointed to the Hobart Professorship of the Latin Language and Literature, a position which he held up to the time of his death. He was a life member of the American Philological Association and of the Archaeological Institute of America, and served for many years as Secretary of the Hobart College chapter of the Phi Beta Kappa Society.

Mr. Woodman was a man of broad learning and culture. He was familiar with the life, languages, and literatures of the principal countries of Europe, having spent considerable time in most of them in travel and study. In his daily life he was modest and retiring, but he will long be remembered by his students and his friends for his many amiable qualities and for the sterling virtues which marked him as a scholar and as a man.

<div align="right">E. J. WILLIAMSON</div>

THE REV. HENRY J. HECK, Teacher of English in the Pontifical College Josephinum (Worthington, Ohio), and a member of the Linguistic Society of America since 1930, died on August 27, 1935, after an illness of two months.

He was born at Union City, New Jersey, on February 28, 1891, and entered the College Josephinum as a student in 1907. In 1918 he was ordained to the priesthood; he afterward received the A.M. degree from Columbia University, for work done in summer sessions, and at the time of his death was completing his studies for the Ph.D. from Duquesne University.

Father Heck was a careful and thorough scholar, whose worth was proved by his conduct of courses at the Josephinum and by his influence with the students. He taught Latin for some years, then in 1930 took over the classes in college English. Of him a colleague writes: 'As a teacher Father Heck was sanely progressive. He held on to the old whenever and wherever he thought it really beneficial. But he did not permit his love of the traditional to interfere with evident need of readjustments. The good of his students came first. His own likes and dislikes, his own convenience or leisure had to be sacrificed.'

WILLIAM FREDERIC BADÈ, Professor of Old Testament Literature and Semitic Languages and Dean of the Pacific School of Religion, and Director of the Palestine Institute, a member of the LINGUISTIC SOCIETY OF AMERICA since 1933, died at his home in Berkeley, Calif., on March 4, 1936, in his sixty-sixth year.

He was born at Carver, Minn., on January 22, 1871, and received the A.B. from Moravian College in 1892; B.D., Moravian College 1894; B.D., Yale University 1895; Ph.D., Moravian College 1898; D.D., Pomona College 1922; Litt.D., Mills College 1925. He taught Greek and German, then Hebrew and Old Testament Literature at Moravian College from 1896 to 1902, and from there went to the Pacific School of Religion, to which he was attached in the capacities mentioned above, for the remainder of his life.

Dr. Badè was a man of wide interests. Apart from the subjects which he taught, he was keenly interested in ornithology and similar topics; he was a friend of the naturalist John Muir, certain of whose writings he edited; he was president of the California Associated Societies for the Conservation of Wild Life, and was active in other similar organizations. He was a member of many learned societies in this country and abroad. But his chief fame rests upon his determination of the site of the lost city of Mizpah, about twenty-six miles northwest of Jerusalem, on the excavation of which he started work in 1926; on this site he uncovered seven successive cities, the latest of which dated from 900 B.C. His last campaign was in 1935; when he returned home late in the summer, his health had been undermined by his labors in the Orient, with the result that he lived only until March 4 of this year.

THE UNIVERSITY OF CALIFORNIA in its Summer Session of 1936, will, jointly with the Institute of Pacific Relations, conduct intensive courses in the Russian language, one course for beginners, and one for those with some knowledge of Russian. The courses will last for ten weeks, June 22 to August 29. The work thus offered is a continuation of the inter-university project which began with the Russian Language Section of the Harvard Summer School in 1934 and was carried on at Columbia University during the summer of 1935. The courses will not treat the language from a scientific standpoint, but will be aimed at giving a practical command of it to persons of exceptional qualifications who need Russian as a tool for use in some field of science or scholarship. Information may be obtained from the office of the Summer Session of the University of California.

DR. SAMUEL E. BASSETT, Professor of the Greek Language and Literature in the University of Vermont, has been appointed Sather Professor of Classical Literature in the University of California, for 1936–37. He will be in residence at Berkeley from January to May, 1937, during which period he has received a leave of absence from the University of Vermont.

DR. N. M. CAFFEE, formerly of Norfolk, Va., has gone to the Louisiana State University as Instructor in English.

DK. CHARLES J. DONAHUE has become Instructor in English in the Graduate School of Fordham University.

DR. MARTHA JANE GIBSON, of the University of Maine, has gone to Barnard College as Lecturer in English.

DR. E. ADELAIDE HAHN, Associate Professor of Latin and Greek in Hunter College, has, by the payment of the statutory sum on December 31, 1935, become a Life Member of the Linguistic Society.

DR. ALICE E. KOBER, Instructor in Classics in Brooklyn College, has been advanced to an Assistant Professorship.

DR. CHARLES EDGAR LITTLE, Professor of Latin in the George Peabody College for Teachers at Nashville, Tennessee, has completed fifty years of association with the College, as student and teacher of classical languages. The Peabody College accordingly devoted its Founders Day Exercises on February 18 and 19, 1936, to the honoring of him and his long services to the institution.

DR. HELEN POPE has been promoted from the rank of Instructor in Classics to that of Assistant Professor, in Brooklyn College.

DR. OTTO SPRINGER, Head of the Department of German at Wheaton College, will go to the University of Kansas in September as Chairman of the Department of Germanic Languages and Literature.

FRITZ TILLER is now Instructor in German at Yale University.

DR. R. WHITNEY TUCKER has gone to Pennsylvania Military College, Chester, Pa., as Professor of Foreign Languages.

DR. W. FREEMAN TWADDELL, until recently Assistant Professor of German at the University of Wisconsin, has been promoted to an Associate Professorship.

DR. JOHN PAUL WENNINGER, Assistant Professor of German in Marquette University, Milwaukee, Wis., was received into the Linguistic Society as a member for 1935, after the last published list.

THE FOLLOWING NEW MEMBERS FOR 1936 were received into the LINGUISTIC SOCIETY subsequent to the last published list, and up to March 9, 1936:

Samuel Ogden Andrew, M. A. (Oxon.), The Hey, Sanderstead, Surrey, England; *classical and mediaeval languages, Germanic languages.*

Albert Croll Baugh, Professor of English, University of Pennsylvania; 317 S. 43d St., Philadelphia, Pa.; *Middle English.*

John Bissell Carroll, undergraduate student at Wesleyan University, Middletown, Conn.; *American Indian languages.*

Procope Sarantos Costas, Ph.D., 6200 University Avenue, Chicago, Ill.; *history of the Greek language.*

Jane E. Daddow, M.A., assistant on staff of Linguistic Atlas of United States and Canada; 56 Olney St., Providence, R.I.

Leah A. Dennis, Ph.D., Associate Professor of English, Alabama College, Montevallo, Ala.

Frederick George Dyas Jr., B.A., Instructor in Spanish and German, Roxbury School; 2066 Yale Sta., New Haven, Conn.; *Old Spanish.*

Hedley Powell Jacobs, B.A. (Oxon.), Strathnor, Strathairn Ave., Half-Way Tree P.O., Jamaica; *Teutonic languages, Creole languages.*

Martin Joos, M.A., Assistant in German, University of Wisconsin, Madison, Wis.; *systematic synchronic linguistics.*

Sherman McAllister Kuhn, Ph.D., Assistant Professor of English, Oklahoma Agricultural and Mechanical College; 214 West St., Stillwater, Okla.

Berthe Marie Marti, Ph.D., Assistant Professor of Latin and French, Bryn Mawr College, Bryn Mawr, Pa.

Albert D. Menut, Ph.D., Professor of Romance Languages, Syracuse University, Syracuse, N. Y.; *Middle French.*

Antonio A. Micocci, Instructor in Latin, Catholic University of America; 13 Fairview Ave., Wyncote, Pa.

Julian Joel Obermann, Professor of Semitic Languages, Graduate School, Yale University, New Haven, Conn.

Dorothy Paschall, Ph.D., 5548 Kenwood Ave., Chicago, Ill.; *classics.*

Ernst Alfred Philippson, Ph.D., Assistant Professor of German, University of Michigan; 1047 Olivia Ave., Ann Arbor, Mich.

L. Arnold Post, M.A. (Oxon.), Professor of Greek, Haverford College, Haverford, Pa.

Pierre M. Purves, M.A., care of E. R. Purves, Architects Building, Philadelphia, Pa.; *oriental studies.*

Philip Scherer, Ph.D., teacher of German at Stuyvesant High School, New York City; 2110 Westbury Court, Brooklyn, N.Y.; *Indo-European etymology.*

Mrs. H. W. Schroeder (Ida Lerey), 464 Heights Road, Ridgewood, N. J.; *technical and literary translations from French, Spanish, Italian.*

Adelaide Douglas Simpson, M.A., Instructor in Greek and Latin, Hunter College; 520 W. 114th St., New York City.

Mack Hendricks Singleton, M.A., Instructor in Spanish and Portuguese, University of Wisconsin, Madison, Wis.

Otto Springer, Ph.D., Professor of German, Wheaton College, Norton, Mass.

John William Stanton, Ph.D., Instructor in History, University of Michigan, Ann Arbor, Mich.; *Chinese, Japanese, Russian.*

Nathan Süsskind, M.Sc. in Ed., Tutor in German, City College, New York City; *Judaeo-German, Indo-European.*

Joseph K. Yamagiwa, A.M., Sub-Editor, Early Modern English Dictionary; Dept. of English, University of Michigan, Ann Arbor, Mich.; *English, Japanese.*

Mrs. Anna Rosina Zollinger, Ph.D.; Assistant Professor of German, Brooklyn College; 7040 Colonial Road, Brooklyn, N. Y.; *German-Swiss dialects.*

THE PERSONAL ENDINGS OF THE MIDDLE VOICE

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One of the questions concerning the reconstruction of IE forms which needs thorough reconsideration because of the new evidence from Hittite and Tocharian is the extent of the development of middle forms in the parent language; more particularly, whether IE possessed both of those sets of endings which are conventionally called 'primary' and 'secondary', and even special endings for the perfect, or whether there was only one, as in Latin and Gothic. As long as Aryan and Greek were the only language groups which had well developed middle paradigms, at least without the *r*-element, it was natural to base on them all conclusions concerning the IE status, and to assume that the parent language also had sets of both primary and secondary endings, but that Italic and Keltic were the innovators in showing no probable trace of the former. However, now that both Hittite and Tocharian are seen to have had complete middle paradigms with and without *r*, and these paradigms agree not with Aryan and Greek, but with the other IE languages in showing only one set of endings, the weight of evidence has shifted to the other side. It is therefore necessary to re-examine the various middle paradigms to determine which features can be traced to a common source, and whether those which are probably not IE can be plausibly explained as originating in the individual languages.

As a first step it will be advantageous to review briefly the existing sets of primary and secondary endings in the various IE languages, so as to balance the probabilities as to which side represents the original status. In Aryan alone do we find the distinction between the two carried through consistently, the primary endings all being characterized by a final *-ē* (IE *-ai*), the others by its absence (except in the thematic 1. sing., as Skt. *á-bharē*). Thus Skt. 1. sing. *ā́s-ē* : *ā́s-i*, 2. sing. *ā́s-sē* : *ā́s-thās*, Av. *parᵊsa-he* : *-zaya-ŋha*, 3. sing. Skt. *ā́s-te* : *ā́s-ta*, 1. pl. *ā́s-mahē* : *ā́s-mahi*, 2. pl. *bhára-dhvē* : *á-bhara-dhvam*, 3. pl. *ā́s-ate* : *ā́s-ata*, *bhárantē* : *á-bhara-nta*, 1. du. *ā́s-vahē* : *ā́s-vahi*, 2. du. *ā́s-āthē* : *ā́s-ātham*, 3. du. *ā́s-āte* : *ā́s-ātām*. In Greek there are three forms directly comparable with the Aryan in using primary endings in *-αι* as opposed to

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secondary endings in -*o* (Ar. -*a*): 2. sing. ἦσαι < *ἦσ-σαι: ἦσο < *ἦσ-σο, φέρεαι < *φερε-σαι: ἐ-φέρεο < *ἐ-φερε-σο, 3. sing. ἦσ-ται: ἦσ-το, φέρε-ται: ἐ-φέρε-το, 3. pl. φέρο-νται: ἐ-φέρο-ντο. In addition the 1. sing. has -μαι, e.g. in φέρο-μαι, as opposed to -μην in ἐ-φερό-μην. In the 1. and 2. pl. Greek has only one ending, and that is the ending which appears as secondary in Aryan. For the former cf. ἤμεθα pres. and imperf., and φερό-μεθα, from which the imperfect ἐ-φερό-μεθα differs only in having the augment. Similarly in the 2. pl. ἦσθε, φέρε-σθε, and ἐ-φέρε-σθε. Also in the 2. du. the same ending is both primary and secondary, cf. ἦσθον, φέρε-σθον, ἐ-φέρε-σθον. In the 3. du. the difference between the present ἦσθον and φέρε-σθον and the imperfects ἦσθην and ἐ-φερέ-σθην is patterned after that of the active -*τον* to -*την*, a change which took place in the Greek language itself. It thus appears that Greek agrees with Sanskrit and Iranian only to a limited extent in showing a distinct set of primary endings, and in only three instances does it look as though the primary forms might be old and inherited.

Squarely against the evidence of the Aryan and Greek is that of Hittite and Tocharian. The former shows a paradigm used as present of which each form ends in -*a*, i.e. IE -*o*, the final sound of three of the most important IE 'secondary' endings. The oldest Hitt. endings are shown by the following: *tapar-ha*,[1] *pahhas-ta, ar-ta, ar-wasta, iya-dduma, es-anta*. There is here no trace of the supposed IE primary endings, and yet the forms all are classified as present, whereas the past is formed by adding a -*t* at the end of the complete present form, e.g., in the 3. sing. *ar-ta-t* beside *ar-ta*. In Tocharian we find a middle paradigm corresponding in general to that of Hittite, but it functions as preterite instead of present, reserving for the present tense the same endings extended by *r*. Thus *kälp-e, kälpā-te, kälpā-t, kälpā-mät, kälpā-c, kälpā-nt*. The 1. sing. has -*e*[2] < IE -*ai* like Skt. *á-bhar-ē*. From the forms ending in -*t* only a dark vowel could have dropped, not an -*e* < IE -*ai*, for before a Tocharian palatal vowel *t* was palatalized to *c*. Consequently the ending -*t* is IE -*to*, -*nt* is IE -*nto*, -*mät* is IE -*medhə*, ə becoming *a* also in Tocharian. Since the 2. sing. in -*te* is not traceable to IE -*sai*, it follows that Tocharian also shows no trace of any IE ending

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[1] See note 6.

[2] In LANG. 9.30 ff. I was misled by this Toch. -*e* < IE -*ai*, erroneously interpreted as primary ending only, to conclude that the single middle paradigm of Hittite and Tocharian was due to merging the IE primary and secondary endings. Recognition of -*ai* as also the latter, invalidates that conclusion, and we must assume that these two languages show no trace of alleged IE primary endings.

classified as primary only. Thus Tocharian also inherited only a single set of middle endings, which, however, it uses in the preterite tense as opposed to Hittite.

Turning to the other IE languages, we find that also Italic shows no distinction between primary and secondary endings, with the single exception of the 3. pers. in Umbrian, which uses -*ter* as primary and -*tur* as secondary, but this is not an old distinction, since both endings ultimately go back to combinations of *r* with the IE 'secondary' endings in -*to*; cf. Buck, Gram. of Osc. and Umbr. 178 f. Latin always uses the same endings for both, and wherever these are derived from the corresponding IE middle endings they are of the secondary group. In the 2. sing. -*re*, as in *seque-re* pres. or *sequebā-re* imperf., comes from the IE -*so* of, e.g., Gr. ἦσο < *ἦσ-σο, and if we subtract the -*r* from the 3. sing. *sequi-tur* we have left -*tu-* = IE -*to*, Gr. -το as in ἐ-φέρε-το. Similarly the 3. pl. *sequu-ntur* with -*ntu-* = IE -*nto* as in Gr. ἐ-φέρο-ντο. Just as the Italic, so also the Keltic (Brugmann Gr. 2.3.662 ff.) shows a set of *r*-endings which are based on the IE secondary endings regardless of the tense in which they are found. It is true that Brugmann (663) raised the question whether the OIr. absolute 3. sing., as *suidigidir*, *sechidir*, and the 3. pl., as *suidigitir*, *sechitir*, were to be traced to IE *-(n)trai* because the *i* of the ultima points to the loss of an original palatal vowel, but he himself suggested that the similar -*mir* of the 1. sing. had an original final -*i* after the active in -*mi*, and this explanation is just as good for the above forms, which could have been influenced by the original -*ti* and -*nti* of the active. Keltic thus shows no probable trace of any IE primary ending, and is to be classified with Italic in pointing to a single set of original endings, those which are classified as 'secondary'.

The scant evidence of the Germanic languages is also in line with that of all the others except Greek and Sanskrit. The old form of the 1. sing. is found only in OIcel. *heite* 'I am called', which shows the same -*ai* as Toch. *kälp-e* and Skt. *bhár-ē* and *á-bhar-ē*; but Gothic has *bairada*, which is the third person used also as the first. In the 2. sing. Goth. *baira-za*, -*za* = IE -*so*; in the 3. sing. *baira-da*, -*da* = IE -*to*; in the 3. pl. *baira-nda*, -*nda* = IE -*nto*, the last being used also for the 1. and 2. pl. It is true that it is a widely held opinion that the final -*a* of all of these forms comes from IE -*ai*[3]; but, as Sturtevant, Hitt. Gram. 265, remarks, the change of final -*ai* to Goth. -*a* is assumed solely to account for these

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[3] So, e.g., Streitberg, Urgerm. Gram. 322. Brugmann (647) is undecided between -*ai* and -*o*.

endings, and in view of the absence of these endings in -*ai* from all languages except Aryan and Greek, their existence in Gothic is anything but probable. On the other hand it may be necessary to assume a long final -*ō* as the source of the Goth. -*a*, which would be a case of IE lengthening of final vowels. But whether we have to assume -*ō* or may assume -*ŏ*, there is no probability of any of the primary endings being represented in Germanic, which agrees with Hittite, Tocharian, Italic, and Keltic, in having only such middle forms as are ordinarily classified as secondary.

Of the remaining IE languages only the Slavic furnishes even the most fragmentary evidence concerning the medio-passive. The OChSl. *vědě* 'I know' (perfect as present) of course has IE -*ai*, but this is found both as primary and secondary as well as perfect ending. Supposed traces of IE primary endings in Baltic are certainly illusive. It has been suggested that in the reflexive of the 1. and 2. sing. of *mi*-verbs IE -*ai* was preserved; that, e.g., in Lith. *dŭ-më-si* the -*më*- is identical with Gr. -*μαι*, and in the 2. sing. *de-së-s* IE -*sai* has been sought in the -*së*-. However, these diphthongs are certainly to be explained by a proportional analogy to the thematic 2. sing.; for, e.g., *sukì* 'thou turnest' had an original final diphthong which is preserved in the reflexive *sukḗ-s*. From here the *ë* went first to the athematic 2. sing.: *de-së-s* : **de-si* (stem *ded-*) = *suk-ḗ-s* : *suk-ì*, then also to the 1. person, as in *dŭ-më-si*. In view of the dubiousness of the existence of IE primary forms in -*ai* the latter must be the correct explanation, which is also supported by the fact that the 3. sing. of reflexive *mi*-verbs ends in -*ti-s*, not -*të-s*; cf. Wiedemann, Handbuch 108. We may conclude that also the Balto-Slavic furnishes no probable evidence for the existence of IE 'primary' middle forms.

Summarizing the situation in all of the IE languages, it may be said that Hittite, Tocharian, Italic, Keltic, Germanic, and Balto-Slavic all show no probable evidence of the IE so-called primary endings of the medio-passive, and that all of these except the Balto-Slavic bear witness to the existence of the IE 'secondary' endings. The evidence for the former is confined to the complete paradigms of the Aryan and the three (or four) forms with final -*αι* in Greek. It seems a legitimate conclusion that IE (exclusive of the imperative[4]) knew only one set of middle endings, and that was the one that was specialized in secondary use in Aryan and (partially) in Greek, so that the latter languages were the

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[4] For the perfect see below.

innovators. To further substantiate this conclusion, which is so far
based on generalities, it will be necessary to reconstruct the single IE
paradigm form by form, and then to show how the Aryan and Greek
double set of endings can be derived from the IE single set.

1. THE FIRST PERSON SINGULAR. Every form of every IE language
that is not clearly a later innovation characteristic of one language or
language group only, points to IE -ai as the only ending for primary
and secondary tenses as well as for the perfect. It occurs in the present
in OIcel. *heite* 'I am called' and in Aryan[5] both in athematic and thematic
verbs; the former, e.g., in Skt. *bruv-é* Av. *-mruvē* : Skt. *brū-tē* 'he speaks',
the latter in Skt. *yájē*, Av. *yaze* : Skt. *yájatē*, Av. *yaza'te*. It is found as
a secondary ending in Aryan thematic verbs, e.g., Skt. *á-bharē*, Av.
-ba're : Skt. *bhárati* 'bears', and as only ending of the preterite in
Tocharian, e.g., *kälp-e*. We meet it again in the perfect in Aryan, Latin,
and Slavic, e.g., Skt. *tu-tud-ē* = Lat. *tu-tud-ī* (active), Skt. *çu-çruv-ē* =
Av. *su-sruy-e*. In Slavic only in the OChSl. *vědě* 'I know'. The con-
clusion to be drawn is that IE knew only one ending for the 1. sing.
middle, and that was -ai. From the fact that no language shows this
-ai in active use in any other tense than the perfect, we must conclude
that Latin and Slavic were the innovators in using it in the perfect
active, in spite of Hirt, Idg. Gram. 4.146 f. To show that it had been
active as well one must also find it used actively elsewhere, for the
affinity between the other middle tenses and an intransitive perfect
active often occurring alongside, offered a continuous inducement for
the middle ending to slip over to the intransitive perfect. Compare,
e.g., Skt. Ved. *ru-róc-a* beside the invariably middle present *rócatē*, Gr.
ἔ-στη-κα 'I stand' beside pres. middle *ἵ-στα-μαι* (the active present is
transitive), perf. *μέ-μον-a* beside pres. *μαίνομαι*, etc.; cf. Delbrück, Ai.
Syntax 235 f.; Hirt, Handb. 566. That such instances could have been
the starting-point for the adoption of the original middle ending as the
regular active ending, is evident.

The only form connected in no way with this -ai is Goth. *bairada*,
which is the 3. sing. used also as first, and took the place of the origi-
nal form, preserved in OIcel. *heite*, under the influence of the plural,
that used *bairanda* for all three persons.

Three language groups show forms which are not immediately com-
parable with those in -ai, but nevertheless presuppose the latter.

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[5] The -āi in the subjunctive, e.g. Skt. *yájāi* Av. *yazāi*, is due to contraction, cf.
Thumb, Handb. d. Skt. 293.

The Hitt. *-ha (also reduplicated *-ha-ha)[6] is to be explained as changing
-e < IE -ai to -a under the influence of the -a of the other middle
endings.   The Aryan athematic secondary forms in -i, as Skt. ás-i, Av.
aoǰ-ī, are due to analogies which can be put in the form of proportions
like the following: ás-i (imperfect) : ás-ē (present) = ás-mahi (imperf.
pl.) : ás-mahē (pres. pl.).   Finally, the Greek primary -μαι, e.g., ἦμαι
< *ἦσ-μαι, φέρο-μαι, has been rightly explained as due to the influ-
ence of the active in -μι on the old middle in -ai; cf. Brugmann, op. cit.
2.3.642.   This -μαι must at first have been secondary as well, but in
later use was changed to -μην under the influence of the intransitive
aorists in -ην, -āν, particularly those which are labelled as passive.
Thus *ἐ-σπειρα-μαι, aorist middle of σπείρω, became ἐ-σπειρά-μην because
of the equivalent ἐ-σπάρ-η-ν[7] (aor. pass.).   The Doric -μāν at first sight
seems unfavorable to this interpretation, since these aorists had IE -ē-,
but it seems that Doric had developed a similar although less extensive
category in -āν, cf. Epid. ἐξ-ερρύᾱ (3. sing.), or ἐφθίᾱ· ἀπέθανεν Hesych.;
cf. Brugmann-Thumb 325.

2. THE SECOND PERSON SINGULAR.   Here conditions are much more
complex.   The form in -sai, as in fact all forms in -ai which are primary
only, will be discussed later.   Of the others the best attested is -so.
As secondary ending it occurs in Avestan, e.g., athematic -aoγ-žā,
thematic -zaya-ŋha, and in Greek, e.g., ἦσο < *ἦσ-σο, ἐ-φέρεο < *ἐ-φερε-σο.
As both primary and secondary it is found in Latin -re[8], e.g., pres.
seque-re as well as imperfect sequēbā-re.   As primary it occurs (with long
-ō?) in Goth. baira-za[9].   That it was used as primary ending is also
shown by Hitt. -ta, for which see below.

[6] These endings are marked with the asterisk because deduced as the necessary
basis for the ri-presents like ya-ha-ri from ya- 'go' and for preterites like es-ha-t
from es- 'sit'.   Actually existing tapar-ha 'I governed' and the like cannot be
cited as direct evidence for the existence of this form.   They are marked with the
'Glossenkeil', and clearly are Luwian preterites borrowed in their entirety; cf.
Sturtevant, Gramm. 264.   Nevertheless the Luwian -ha must be ultimately identi-
cal with the Hitt. *-ha, but the former apparently did not develop a secondary
distinction between presents without final -t and preterites with it.

[7] Influence of aorists in -ην is also suggested by Gray, LANG. 6.231, but otherwise
his interpretation of the ending -μην differs widely.

[8] Also in the OLat. form in -rus, e.g. experīrus < *-so-s, made by adding the -s
of the active before the change of -so to -re, cf. Brugmann 647.

[9] That Goth. here retained the old special form in -za opposed to the 1. sing.
and the 1. and 2. pl., which adopted the forms of the 3. person, is explained by
Van Helten, IF 14.89, as due to the support -za (according to him -zai) had in the
-zi of the 2. sing. active.

The evidence for IE *-thēs* is not so clear, but sufficient to make its existence plausible.   Not only Skt. *ás-thāḥ* as well as thematic *á-bhara-thāḥ*, but also Keltic shows the ending, which became *-the* or *-de*, in the imperative of deponent verbs, e.g., OIr. *comaln(a)i-the* or *-de*.   Extended by *-r* it occurs also in the indicative present, as absol. *suidig-ther*; cf. Thurneysen, Handb. d. Air. 342. IE *-thēs* is also sought in the 2. sing. in *-θης* of Greek aorist passives; e.g., *ἐ-δό-θης* is identified with Skt. *á-di-thāḥ*, and *-θης* is supposed to have been a single personal ending originally instead of stem-suffix *-θη-* with personal ending *-s*; cf. Brugmann, op. cit. 172 f.   This is of course merely a possibility[10], but we can find a more certain trace of IE *-thes* in Hitt. *-ta*, as in *pahhas-ta*.   This *-ta* was a contamination of pre-Hitt. *-tes* < IE *-thēs* with *-sa* < IE *-so*.   Similarly Tocharian *-te*, e.g., in *kälpā-te*[11], except that *-e* was added[12] by analogy to the 1. sing. in *-e* < IE *-ai*.   It is to be observed that Hitt. *-ta* serves as present, while the preterite adds a *-t* at the end, and the Tocharian *-te* is preterite only, an *r*-form functioning as present.

We may conclude that IE knew both *-so* and *-thēs* in both primary and secondary function, whereas *-sai* of the Aryan and Greek was a later innovation.   That *-thēs* was originally used for athematic stems, and *-so* for thematic forms, is a probable guess, but nothing more.

3. THE THIRD PERSON SINGULAR.   Again the primary form in *-tai*, characteristic of Aryan and Greek, will be left till later.   The form in *-to* on the other hand occurs in every language group that has a middle paradigm at all.   In Aryan and Greek only as secondary ending, e.g., Skt. *ás-ta*, *á-bhara-ta*, Av. *-aog°-dā*, Gr. *ἦσ-το*, *ἐ-φέρε-το*.   In Hittite, on the other hand, it occurs as a primary ending, e.g., in the present *ar-ta*.   That it originally was used also as a secondary ending is shown by the preterite, e.g., *ar-ta-t* or *es-ta-t*, which is made by adding the secondary ending of the 3. sing. act., sc. *-t*, to the old form in *-ta*, in the creation of a new past tense.   Tocharian has IE *-to*, e.g., in *kälpā-t*, the final *-a* < IE *-o* having dropped as all final short vowels do.   That this vowel was a dark vowel, and not *-e* or *-i*, is shown by the fact that the *t* has not been palatalized to *c*.   The Tocharian form in *-t* was used

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[10] Even more uncertain is the identification of the *-dēs* of the 2. sing. of Germanic weak preterites, as Goth. *nasidēs*, with IE *-thēs*; so, e.g., Streitberg, op. cit. 339 f., for primary verbs.

[11] Toch. *kälpā-te* won out over the older **kälpā-t* because the latter had become identical with the 3. sing. < IE *-to*.

[12] That the addition of *-e* took place after the separate existence of the Tocharian language, is proved by failure of the *t* to become palatalized to *c*.   Contrast, e.g., *ce-m* = IE **toi* with added *-m*.

as preterite, but it must also at one time have been a present, for the pres. *kälpā-tär* is an *r*-form made by adding *-är* to *kälpā-t*. In Latin, *-to* is the basis of *-tur*, made exactly like Toch. *kälpā-tär*, by adding *-r* to the old 3. sing. It is primary in the present *sequi-tur*, secondary in the imperfect *sequēbā-tur*. Oscan-Umbrian also shows evidence for the ending *-to*, e.g., Osc. *vincter* 'vincitur', **sakarater** 'sacratur', Umbr. *herter* 'oportet'. The *-ter* seems to be derived from *-tro*, patterned after the plural *-ntro*, which was a contamination of *-nto* and *-ro*, cf. Brugmann, op. cit. 2.3.661. Finally, Gothic *baira-da* has *-to* (*-tō* ?) in the present. It scarcely admits of doubt that *-to* was from IE times used both as primary and secondary ending, and that Skt. and Gr. *-tai* was a later development.

4. THE FIRST PERSON PLURAL. Here only the Aryan has a distinctly primary form in *-ē*, Greek agreeing with the others in having only one form. After understanding that the Aryan primary forms are secondary creations, and that there is no old ablaut between primary *-mahē* and secondary *-mahi*[13], it becomes clear that IE knew only one ending, and that this was *-medhə*, which is now established by the Tocharian in addition to Aryan and Greek. Compare, e.g., Skt. *ás-mahi*, *á-bharā-mahi*, Av. *var°-ma⁺dī*. In Greek it is both primary and secondary, e.g., ἥμεθα is both present and imperfect, φερό-μεθα pres., ἐ-φερό-μεθα imperf. Also Toch. *-mät*, e.g., in *kalpā-mät* (preterite) comes directly from IE *-medhə*, and the fact that the final *-t* is not palatalized to *-c*, proves conclusively that an *-a* < IE *-ə*[14], and not an *-i* was lost after the *-t*. The only other language which has a distinct form for the 1. pl. which is not in its entirety an innovation, is Hittite, which has the ending *-wasta*, e.g., in *ar-wasta*. This probably took the place of an older *-*masta*, changing to *-wasta* along with the change of *-*me* to *-*we* (originally dual) in the 1. pl. active. Hitt. *-*masta* was a contamination of *-*mata*[15] = IE *-medhə* with the *-mos* of the active, found, e.g., in Lat. *-mus*, as *agi-mus*.

5. THE SECOND PERSON PLURAL. Again Aryan stands alone in having a special primary form in *-ē*. All of the others can be derived from IE *-dhyem* or the corresponding weak grade *-dhum*. The former

[13] Even without the evidence of Tocharian, Güntert, Idg. Ablautsprobl. 12, concluded that Skt. *-mahē* must have been a more recent creation than *-mahi*.

[14] After IE *-medhə* has been definitely established by Toch. *-mät*, it is unnecessary to take further cognizance of older opinions which held that Gr. *-μεθα* could not have been old. So Holger Pedersen, KZ 36.80; Hirt, IF 17.67.

[15] The first *a* in place of IE *e* is a case of vowel assimilation, cf. JAOS 54. 161 ff.

certainly as a secondary ending in Aryan, as Skt. *ádhvam*, *á-bhara-dhvam*,
Av. *mạz-daz-dūm*, *dāraya-δwəm*, the latter apparently in Hitt. *-duma*, as
iya-dduma (pres.), with final *-a* from other middle forms.   It is also
possible that *-dum* is a purely Hittite development from *-dhųem*, and
even that *du-ma* represents a cuneiform way of writing *dvema*, cf.
Sturtevant, Hitt. Gram. 58 ff.   Also Toch. *-c* of, e.g., *kälpā-c* (preterite)
probably comes from *-dhųem*[16].   The numerals *ṣpät* < IE **septm̥*
and *śäk* < IE **deḱm̥* show that final syllables containing *-m* may dis-
appear altogether in Tocharian; and the *-c*, product of the palatalization
of *t* (here from IE *dh*), shows that the ending originally contained a
palatal vowel, and points therefore to an IE *-dhųem*, not *-dhųom* or
-dhum.   Again it is to be observed that both Hittite and Tocharian
forms must originally have been primary as well as secondary, for the
Hitt. preterite again merely adds a *-t* to the present form (cf. *kis-duma-t*
pret. with *iya-dduma*), and the Tocharian present adds *-är* to the pre-
terite (cf. *kälpā-c-är* and *kalpā-c*).   The only other language with a 2.
pl. middle which contains old elements is the Greek, which uses *-σθε*
as both primary and secondary ending, e.g., *ἦσθε* (present and im-
perfect), *φέρε-σθε* (pres.), *ἐ-φέρε-σθε* (imperf.).   It is generally rec-
ognized that this must come from **-dhųe* with preceding *s*, and so
also points to the *e* of IE *-dhųem* and not to *-dhųom* or *-dhum*.   There
are two features of the Greek *-σθε* that require explanation.   The
omission of the final *-ν* < IE *-m* is doubtless due to the active 2. pl. in
-τε[17], so that *φέρεσθε* rather than **φερεσθεν* stood beside *φέρετε*.   For
the addition of the *σ* I propose the following simple explanation
instead of the exceedingly complex processes assumed in Brugmann-
Thumb 407 and Hirt, Handb. 493 f.   This may be represented by
the proportion: *ἤμεθα* (< **ἤσ-μεθα*) : *ἦσ-θε* = *διδό-μεθα* : *δίδο-σθε*.   In
other words, the pattern for the *σ* of *-σθε* was some verb with theme
in *-σ-*, which lost the *σ* before the *μ* of the first persons singular and
plural.   As a result the closely associated 2. pl. appeared to contain a *σ*
which belonged to the ending, and other verbs were patterned after the
σ-stem.   This presupposes the existence of a very important *s*-stem, such
as IE *ēs-* 'sit' had been, and Gr. *ἦσ-* must have been at one time, and the
compound *κάθ-ημαι* continued to be.

As a result of the consideration of the various forms examined we may
conclude that IE had only one ending for the 2. sing. middle, and that

[16] Cf. Lang. 9.31.
[17] According to Wackernagel, KZ 33.57, the entire ending *-σθε* is an analogical
creation beside act. *-τε*.

was the 'secondary' -*dhyem*, perhaps alternating with its weak grade -*dhum*.

6. THE THIRD PERSON PLURAL. Greek again agrees with the Aryan languages in having a special primary form in -*ai*, which will be discussed below. All other evidence points to IE -*nto* after vowels and -*ṇto* after consonants. In secondary function it appears in Skt. *ás-ata* (< -*ṇto*), *á-bhara-nta*, Av. *var-atā* (< -*ṇto*), *yazə-nta*, Gr. *ἥατο* (< -*ṇto*), *ἐ-φέρο-ντο*. Hittite and Tocharian use it parallel to -*ta* of the singular, whence the Hitt. presents *ar-anta*, *es-anta*, and the Toch. preterits like *kälpā-nt* (< -*nto*). With the regular extension for the forms of the contrasting tenses it is found in Hitt. preterits of the type *ar-anta-t* and in Toch. presents like *kälpā-nt-är*. Gothic has -*nto* (or -*ntō*) in the thematic *baira-nda*[18]. In Italic and Keltic -*nto*, as -*to* in the singular, was the basis for the *r*-forms. Latin has -*ntur*, which is -*nto* with added -*r*, in both primary and secondary tenses, e.g., *sequu-ntur* pres. and *sequēba-ntur* imperf. The same -*nto* is the basis of the Oscan-Umbrian and Keltic forms which are derived from -*ntro*, a contamination of -*nto* and -*ro*; so, e.g., Osc. **karanter** 'vescuntur', Marruc. *ferenter* 'feruntur', Ir. *do-moinetar* 'they think'.

Evidently, then, the IE knew only one ending, -*nto* with its phonetic variant -*ṇto*, for the 3. pl. middle, and Sanskrit, Iranian, and Greek were again the innovators in developing a special primary ending -*ntai*.

7. THE DUAL. Since Hittite and Tocharian both do not have dual forms of the verb, it is still true that Aryan and Greek are the only languages which have any dual forms of the middle at all, and it is still impossible to reconstruct an IE dual of the middle because these two do not agree. However, we do have now the right to assume that IE knew no special primary dual forms of the middle any more than it knew them in the singular and plural, and that Sanskrit and Iranian were herein also the innovators.

As far as Greek is concerned, it has been clear that all dual middle forms are analogical[19] creations. After the relation of the 2. pl. act. pres. *φέρε-τε* to *φέρε-τον*, the corresponding 2. and 3. dual, the middle 2. pl. *φέρε-σθε* induced the 2. and 3. du. *φέρε-σθον*. For the distinc-

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[18] The same form functions also as first and second person. It has been suggested that in the former use it came from IE -*medhə* with syncopation of the middle syllable and change of *m* to *n* before the dental. However, the assumption of such a syncope is dubious, and somewhere in the process analogy must have played a part, cf. Van Helten loc. cit. (note 9, above).

[19] See Brugmann-Thumb 409.

tion between secondary -σθην and primary -σθον in the 3. du., as ἐ-φερέ-σθην imperf. beside φέρε-σθον pres., the model was furnished by the corresponding distinction in the active, as ἐ-φερέ-την beside ἐ-φέρε-τον. In the 1. pl. the rare -μεθον, which occurs three times in the older poets, e.g. in περι-δώμεθον, is certainly adapted from the plural -μεθα after the duals in -σθον.

Of the Aryan 'secondary' dual forms, the first person may be derived from an IE -ᵫedhə, a form supported by the 1. pl. in -medhə; cf. Skt. á-bharā-vahi, Av. d-vaⁱdī. It is possible that this -ᵫedhə beside the corresponding active -ᵫe was an analogical formation after the pattern of the plural -medhə beside -me, but this may as well have taken place in IE times as later. At all events IE -ᵫedhə has a further possible support in Hitt. -wasta,[20] the ending for the 1. pl. which was explained above as changed from *-masta. It is possible that this was in its entirety originally a dual ending which later became plural when the Hittite dual disappeared. In that case Hitt. *-wata < IE -ᵫedhə became -wasta through contamination with *-was = IE -ᵫos of the dual active.

Whether the Aryan secondary dual forms of the 2. and 3. persons, e.g. Skt. āsāthām and á-bharēthām in the second person, and Skt. ásātām, á-bharētām, Av. a-srvātəm, ǰasaētəm in the third, go back to IE origins, there is no way of deciding, although the age of the secondary endings in general makes it probable.

The reconstructed IE paradigm of middle endings, which made no distinction between primary and secondary endings, had the following appearance:

Singular	Plural	Dual
1. -ai	-medhə	-ᵫedhə (prob.)
2. -so, -thēs	-dhᵫem (-dhum?)	-?
3. -to	-nto, -n̥to	-?

As indicated above, the conclusions which have been drawn must be confirmed by an examination of the Aryan and Greek primary endings, in order to determine whether these can be explained on the basis of this paradigm and such changes as might have taken place in the individual languages, or, eventually, by both of these groups in common. If such explanations could not be made plausible, it would be a strong argument against the conclusions just established concerning the IE status.

[20] Sturtevant, e.g., Hitt. Gram. 266, supposes that the w belonged to the original plural (instead of dual) ending.

Taking up first the Greek, where conditions are more transparent, reference has already been made to the 1. pers. in -μαι as being a transformation of the IE form in -ai under the influence of the present active in -μι.   The latter being a primary suffix only, the associated -μαι also became restricted in the same way, in contrast to -μην, which had past associations carried over from the type ἐσπάρην from which its -ην was probably derived.   After this beginning the rest is clear. The -αι went from the first person of the singular to the second and third, and from the latter to the closely associated third plural.   We can represent all of these changes by a compound proportion: -μαι : -μι = -σαι : -σι[20] = -ται : -τι[21] = -νται : -ντι.   In the 1. and 2. pl. the old undifferentiated forms continue also as primary, e.g. φερό-μεθα, φέρε-σθε.

In the Aryan languages the development of primary forms in -ē (IE -ai) has been more complete, and conditions leading to their development more complex.   As the first stage it may be assumed that the old -ē of the 1. sing., as Skt. āś-ē, Av. yaz-e, slipped over to the 2. sing., which is more closely associated with it than any other form. Consequently, e.g., Skt. āś-sē, bhára-sē, Av. pər°sa-he.   It was exactly the same thing which happened in Tocharian when the -e went from the 1. sing. kälp-e to the 2. sing. kälpā-te for older *kälpā-t (see above).   In Aryan, however, these new forms in -sē were associated with the primary active 2. sing. in -si, with which they appeared to be in ablaut relation. Consequently -sē was specialized as primary ending and left -sa and -thās to secondary use.   Then, e.g., after the pattern bhára-sē to bhára-si also in the third person bhára-tē beside the active bhára-ti, and after the latter in turn the 3. pl. bhára-ntē beside bhára-nti.   The primary 1. pl. in Skt. -mahē, Av. -ma̍de was due to the combined influence of the old 1. sing. in -ē and the primary 3. pl. in -ntē, assisted by the fact that the new form with its -ē appeared to be in regular ablaut relation to the older secondary -i of -mahi.   It is to be observed that the latter factor could have worked only after the change of IE ə to Aryan i, for neither IE ai and ə nor Aryan ē and ə could have been felt as standing in such relation.   This speaks for the late origin of the ending: Skt. āś-mahē, bhárā-mahē, Av. yaza-ma̍dě, etc., could not have been developed until after the end of the IE period.   In the next place -ē invaded the 2 pl., e.g. Skt. ádhvē < *ās-dhvē, bhára-dhvē, Av. dīdraγžō-duyē,[22] as opposed

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[21] Since unchanged -σι in the 2. sing. is found only in ἐσ-σι, and -τι only in West-Greek, the whole process must have been completed in the earliest stages of the Greek language and before its dialectic differentiation.

[22] For Av. -yē = -vē see Reichelt, Av. Elementarb. 72.

to the secondary *á-bhara-dhvam*, etc. It is not certain whether the creation of *-dhvē* beside *-dhvam* should be ascribed merely to the combined influence of the 1. pl. in *-mahē*, the 3. pl. in *-ntē* or *-atē*, and the 2. sing. in *-sē*, or whether at one time **á-bhara-dhva* existed beside the equivalent *-dhvam* (cf. Gr. *-σθε*), so that *-dhvē* could be explained by proportional analogies to forms originally ending in *-o*. Thus *bára-dhvē* : **a-bhara-dhva* = *bhára-ntē* : *á-bhara-nta*. If the latter is the correct assumption, it is another proof of the lateness of the Aryan primary middle endings, for the proportion would hold good only after the change of both IE *o* and *e* to Aryan *a*.

In the dual the Skt. *ás-vahē*, *bhárā-vahē* show the same relation to the secondary *-vahi* of *ás-vahi* etc. as the 1. pl. *ás-mahē* to *ás-mahi*, with the probability that the former was patterned after the latter. Finally, also the 2. and 3. dual developed primary forms in *-ē*; cf. e.g., for the former Skt. *ásāthē*, *bhárēthē*, for the latter Skt. *ásātē*, *bhárētē*, Av. *visaēte*. Since the origin and interrelation of the corresponding secondary forms, as 2. pers. Skt. *ásāthām*, *bhárēthām*, 3. pers. Skt. *ásātām*, *á-bharētām*, Av. *a-srvātəm*, is obscure, it is impossible in this case even to surmise the exact process by which the *-ē* could have made its way to them.

The question whether it would not be possible for Aryan and Greek to have developed their primary middle paradigms in common, must be answered in the negative in view of the wide difference between them both in the scope of the innovations and the conditions under which they occurred. At the most it would be possible that the three forms which both had in common, those in *-sai*, *-tai*, and *-ntai*, were common developments; but since there are no other innovations which the two must have performed jointly, and there is therefore no reason for assuming a period of Graeco-Aryan unity, and since all forms can be explained on the basis of the languages in which they occur, it is safe to assume a distinct development for each.

Opposed to these conclusions is that of Sturtevant first expressed in LANG. 7.246 ff., who believes that *-tai* was one of the oldest of middle endings, and found not only in Aryan and Greek, but also in Hittite. He maintains that Hitt. *-ti*, which occurs appended to *-ta* in the 2. sing. pres., e.g. *ar-tati* beside *pahhas-ta*, and frequently beside *-t* in preterites of the medio-passive, e.g. *kis-antat* beside *kis-antati* (3 pl.), is the IE *-tai*, which is described as a sort of free lance originally, wandering around aimlessly for a long time, and finally finding a home in the primary tenses of Aryan and Greek. He also sees this *-tai* in the form *lukatte* or *lukatti* (beside *lukatta*), which has been considered an adverb[23]

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[23] Cf. Sommer, BoSt. 7. 22 ff.

meaning 'the next morning' or 'the next day'.   It will be necessary to go into details in order to evaluate the validity of these combinations.

Since IE *ai*, in common with the other *i*-diphthongs, became *e*, and not *i* (cf. Sturtevant, Gram. 99 ff.), it would be up to Sturtevant to find examples of the spelling -*te* instead of -*ti* to support his theory, but -*te* never occurs in any form that is known to be verbal, nor in one that conforms to the Hittite conjugation.   There are two instances of the spelling *lukatte* beside more frequent *lukatti*, but to draw such a far-reaching conclusion about indisputable verb forms from one that is otherwise classified as an adverb, does not inspire confidence.   Sturtevant supports his contention that *lukatte* is a stereotyped 3. sing. middle pres., with a reference to the fact that the related *lukatta* is repeatedly introduced by the conjunction *mahan* or *man* 'when'; but what holds good for the latter does not necessarily hold good for the former, and in fact the complete absence of these conjunctions when *lukatte* or *lukatti* is used shows distinctly that they were not of the same nature, that the latter were not verb-forms even if *lukatta* was.   The difference between the two is further brought out in their structure: -*ta* is the regular ending for the verb form presupposed, -*te* or -*ti* is unknown.   Finally, *lukatte* as an adverb is so transparent etymologically that it seems adventurous to throw its etymology overboard in a quest for a more subtle and complicated explanation.   It is clear that *lukatte* is a stereotyped dative-locative of an *a*-stem noun *lukatta-*, and that this corresponds sound for sound with Goth. *liuhaþ*, OSax. *lioht*, OEngl. *léoht*, OHG *lioht* 'light', all of which come from an IE *$leu\hat{k}o$-to-m*.   If the existence of IE -*tai* in Hittite depends upon *lukatte* it has nothing upon which to stand.

But how about the indisputable preterite verb forms in -*ti*?   In the first place, since no -*te* is found alongside, they cannot come from IE -*tai*, but must have an old -*i*.   Furthermore, this combination -*ti* cannot be old, for two reasons.   If it had come from IE -*ti*, it would have become -*zi*[24], as in the 3. sing. pres. *ep-zi* < *ep-ti*.   The *i* could have been added to the *t* only after *ti* had become *zi*, which is after the separate existence of the Hittite language.   Moreover, the fact that -*ti* is a late conglomerate of *t* and *i*, is shown by the Hittite forms themselves, for in most instances a form in -*t* occurs beside the one in -*ti*.   Thus in the pret. 3. sing. *esat* or *esati*, in the 2. sing. *artat* or *artati*, in the 3. pl. *esantat* beside *esantati*.   This fact alone would speak for an accretion of two elements in -*ti*, the recent origin of which cannot admit of doubt.   Without repeating the details of the explanation given in AJP 53.206

[24] Cf. Sturtevant, Gram. 126.

ff., it may be said here that in general the final -*i* spread from the active presents in -*mi*, -*si*, -*zi*, -*nzi* to the middle present *r*-forms in -*ri*.   Forming an association with the middle voice in these surroundings, the -*i* also sometimes went to the preterite middles which regularly ended in -*t*.   As a result of all of this we may conclude that there was no unified ending -*ti* in Hittite, and still less did it know the supposed IE middle ending -*tai*.   The conclusion reached above concerning the late origin of -*tai* in Aryan and Greek is confirmed by its absence from Hittite.

Once more, then, the IE knew nothing of the primary endings of the middle, which were special developments of Aryan and Greek, but had only the endings which were labelled as 'secondary', which it used also as primary endings.   However, there is still the question whether there was a special set of perfect endings in the middle.   A priori we would expect not, since those instances where a perfect active is found alongside of middle present and aorist systems seem to indicate that this was the old and original state of affairs, and that the perfect middle wherever and whenever found is a comparatively late growth.   In the same direction points the fact that perfect middle endings are generally the primary endings which themselves have been shown to be late developments. There are in fact only two endings, those in the Aryan languages, which are characteristic of the perfect, and these are easily shown to be innovations.   In the 3. sing. the ending is -*ē* instead of -*tē* in the present, so e.g. Skt. *dadhḗ*, Av. *daⁱδe*, Skt. *bu-budh-ḗ*.   This is merely the old ending of the 1. sing. middle, which was also found as primary and secondary ending.   The pattern for this transfer was the perfect active, in which both forms had -*ā(u)* or -*a*, e.g., *bu-bódh-a* is both 1. and 3. sing.   Again the process must have taken place during the Aryan period after IE *e* had become *a*, for only after that were the two pattern forms identical. The only other distinctively perfect middle ending is the Aryan 3. pl., e.g., Skt. *cakriré*, Av. *čāxrare*.   The -*rē* is a cross between primary middle endings in -*ē* and the *r*-ending of the active 3. pl. perf., as Skt. *dadúḥ* < -*ur* or Av. *vaonarǝ*; cf. Brugmann, op. cit. 2.3.661.

An estimate of the extent of the existence of middle forms in the IE period would not be complete without expressing some opinion concerning the *r*-forms, although an extensive discussion is out of the question in this article.   Recently it has been assumed[25] that the existence of *r*-paradigms in Hittite and Tocharian proves their inheritance from IE. It seems to me, however, that several considerations give warning not to be too hasty, and point very distinctly to the formerly prevalent

[25] So Meillet, BSL 32.1 ff.; E. F. Claflin, AJP 48.157 ff.; Lang. 5.232 ff.

opinion that all middle *r*-forms were ultimately derived from a single form, the (probably impersonal) volitive 3. sing. in -*r*, the form found, e.g., in Osc. **sakrafir** 'let there be consecration', Umbr. *ier* 'itum sit', Ir. *canar* 'there shall be singing'. In the first place these volitive forms (here not impersonal) are now found also in Hittite and Tocharian, e.g. the 3 sing. imper. *es-aru* 'he shall be seated' in the former, the regular imper. sing., as *p-kām-ār* 'carry', in the latter. Moreover, these forms without any other consonant except *r* are the only forms of the Hitt. and Toch. middle with *r*-endings which are not or have not been detachable[26], which do not show alongside them with the same meaning otherwise identical forms without the *r*. This can mean only that the other *r*-forms were made by later adding the -*r* to the *r*-less forms. Finally, the Italo-Keltic *r*-forms have common features which differ from the Tocharo-Hittite, and show that the development on the western periphery of the IE territory was quite distinct from that on the eastern. Thus, while the latter have complete *r*-paradigms, both Italic and Keltic do not show *r*-forms in the 2. pl. Furthermore, the structure of Tocharo-Hittite *r*-forms, as just observed, is transparent, the *r*-ending being added to the complete otherwise identical form, but in Italo-Keltic there are no corresponding forms without *r* at all, and the existing *r*-forms often cannot be explained without assumption of early contaminations (see above). There is thus every reason for adhering to the older opinion that all medio-passive *r*-forms have developed in the individual language groups from a single IE volitive third singular in -*r* (or -*ro*?). That this form was impersonal, is not certain, but probable, in view of the fact that in this way can be found a connection between the *r*-passive and the indisputably IE 3. pl. act. perfect, as in Skt. *dadúḥ* or Hitt. *es-er* 'fuere'. It is still true that, as far as Italo-Keltic is concerned, the details of the process are largely obscure, but their nature in general, in spite of the increased knowledge from Hittite and Tocharian, is still best described by Brugmann, op. cit. 2.3.657 ff. It might be added that the development of the middle *r*-paradigms from a single form has a parallel in that of the regular primary endings of Aryan and Greek from the single form in -*ai*.

We may conclude, then, that Indo-European had only the one set of middle endings which is classified as 'secondary', and in addition a single volitive form in -*r* for the third person singular, probably impersonal originally. This conclusion is in definite conflict with the

[26] For details see LANG. 9.14.

increasing complexity of the IE paradigms reconstructed by some scholars[27], e.g., by Gray, LANG. 6.229 f.   I have not discussed the latter's opinion about individual forms.   That would lead nowhere because the fundamental assumptions according to which Gray's reconstructions have been made are almost diametrically opposite to those which are the basis of my conclusions.   These differences of opinion concern mainly the problems of ablaut involved in the reconstruction of personal endings as everywhere else.   Are we at liberty to assume that because a certain pair of vowels or diphthongs alternate as ablaut variants some-where else, they MUST also have alternated wherever one or the other has been found?   If we answer that question affirmatively, Gray's conclusions will appear the more probable.   The present writer takes the position that in the matter of ablaut actual occurrence is the only reliable guide.   The way to establish a form in a certain ablaut grade is not to deduce it theoretically from some other form, but to show that this particular form in this particular ablaut-grade existed somewhere where it is not patently an innovation.   This seems to me to be the only procedure in accordance with sound historical method, and all generali-zations should be based on what has actually thus been found.   We must bear in mind that the various factors that produced ablaut were evidently not at work at one and the same time, that all of them worked only for a time and then ceased, that later cases are merely analogical

[27] Cf. also the amazingly subtle and complicated reconstruction of IE active and middle forms made by Roberts, LANG. 11.220 ff., to account for the OHG active 1.pl. on the basis of the IE middle.   To me the difficulties of his theory exceed many times those of some of the others which he has rejected.   It is particularly hard to see any serious objections to the interpretation of Brugmann, KVgl. Gr. 591, who thinks of the long $\bar{e}$ of $beram\bar{e}s$ as being derived from the secondary $-m\bar{e}$.   That this existed in Germanic with a long vowel as well as a short one, is shown by the Goth.1.pl. optative $nimaima$ ($< -m\bar{e}$ or $-m\bar{o}$) beside the pret. indicative $n\bar{e}mum$ ($< -m\bar{e}$ or $-m\bar{o}$).   If now we assume that the older form of $-m\bar{e}s$ was $m\bar{e}si$, we can derive the latter by a proportional analogy going back to the time when $-m\bar{e}$ and $-m\bar{e}$ existed alongside of each other: $-m\bar{e} : -m\bar{e}si = -m\bar{e} : -m\bar{e}si$. The ending $-m\bar{e}si$ with short e is identified with $-masi$, e.g. in Ved. Skt. s-$m\acute{a}si$, $bh\acute{a}r\bar{a}$-$masi$, Av. $-bar\bar{a}$-$mahi$, and with the ending of Ir. $ammi < $*$es$-$mesi$.   If one balks at the notion that this old form should have persisted only in Old High German of all Germanic languages, it is easy to assume that it was created inde-pendently as a dialectic pre-Germanic form in what later became OHG territory. The analogy by which $-i$ slipped from the 3.pl. in $-nti$ and the 1.sing. in $-mi$ to the 1.pl. in $-mes$ to change it to $-mesi$, is so easy that it could have been performed repeatedly in different communities without geographical contact.

extensions[28] (cf. Germ. *schreiben, schrieb, geschrieben,* although borrowed
from Lat. *scrībo*), and such analogies never work with consistency,
since the associations at their basis are very complex and vary under
different conditions.   As far as the inflectional system of IE is concerned,
it seems to me more and more evident that a considerable portion of it
grew up in the latest Indo-European period, and quite a few supposed
IE forms were even developed in the individual languages.   This means
that a large part, and probably the larger part, of the IE inflectional
system arose after all of the ablaut-producing factors had ceased work-
ing.   It is therefore necessary to use the greatest caution in operating
with ablaut when dealing with IE inflectional endings.   The deductions
made above concerning the simplicity of the IE middle paradigm are
merely one of various considerations which tend to prove that ablaut[29]
played only a subordinate part in the genesis of the IE inflectional
system.[30]

[28] Thus according to the above exposition the supposed old ablaut between *ai*
and *i* in Gr. δίδονται : Dor. δίδοντι and Skt. *dádhate* : *dádhati* has turned out to be
not IE ablaut, but a late analogical growth in Aryan and Greek.

[29] This means, of course, ablaut in the inflectional endings.   Since the words
themselves largely belonged to older periods, it is inevitable that they should show
ablaut variations of all kinds in their radical parts.

[30] [One fact seems to have been overlooked.   It is true that in Greek before the
use of the augment became obligatory φέρεσθον, φερόμεθα, φέρεσθε were wide
forms, indifferent to the present-past distinction.   The same, however, is true
for the corresponding forms of the active φέρετον, φέρομεν, φέρετε.   In these forms a
present-past distinction is valueless, since all designate an action performed by
the person addressed, who consequently needs not to be told about its time.   Then
a similar wide form is to be expected for the second person singular.   In the
middle there are only narrow forms φέρεαι : φέρεο; but the active with its use of
-*s* both as a 'primary' and 'secondary' ending points to the existence at one time
of wide forms for this function too.   Indeed τίθης, δίδως, ὄμνῦς, φής, φέρες (Dor.
Cypr.) are just what is to be expected (contrast Brugmann-Thumb 398),
provided that the earliest types survive without analogical replacements.

I do not intend to attempt to trace the various ways in which the present-past
distinction received formal expression.   But the wide forms are evidently archaic,
survivals from a time when the language, as Hirt (Idg. Gram. 6.209) believed,
did not distinguish by verbal forms present and past tense.   The differentiation
began, no doubt, in forms of the third person, where the need for it was greatest.
Whether the speaker means 'I am carrying' or 'I was carrying' must frequently
have been obvious from the context; and Hirt is probably right in his suggestion
(Idg. Gram. 4.138) that *-*mi* is analogical to -*ti*, -*nti*.

One corollary may be mentioned: *esi* is not in some obscure way a phonetic
continuant of *es-si*; both are analogic remakings of *es(s) < *es-s.   GMB]

GREEK ἀτύζομαι, A HITTITE LOANWORD, AND ITS RELATIVES

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No satisfactory etymology has been given of Greek ἀτύζομαι 'to be distraught (from fear), amazed, bewildered', also 'to be distraught (with grief)' and, with accusative, 'to be amazed at (a thing)'. This verb is generally used in the passive voice; its rare active, ἀτύζω 'to strike with terror', may be considered an informal causative of the basic intransitive 'to be distraught', though the latter is naturally, from the strictly formal standpoint, merely a passive of the active. A derivative adjective is ἀτυζηλός 'terrifying'. The future of the active, ἀτύξω, and the passive aorist participle, ἀτυχθείς, show clearly that ἀτύζομαι is a -yo- derivative from a basic *ἀτυγ-. There is no such Greek base quotable from forms lying outside the verb paradigm, apart from the secondary form ἀτυζηλός. ἀτύζομαι may also conceivably be a simple thematic denominative from an archaic *i*-stem *atugi- : *atugy-o-mai, but there is no such stem available in Greek.

Nor is there at all obviously elsewhere in Indo-European, aside from Hittite. The etymologies hitherto suggested and half-heartedly quoted by Boisacq[1] need not detain us, for they are hardly more than ad hoc constructions, with little or no inherent probability. To analyse *ἀτυγ- into a 'prefix' ἀ- (of unknown meaning, possibly referable to *ṇ- < *en- 'in' or to *sm̥-) and to equate the decapitated -τυγ- with *tuj*- of Sanskrit *tujáti, tuñjáti* 'he beats, pushes, strikes at' is neither convincing formal analysis nor good sense semantically. It is better, as so often, to recognize a peculiar word or formation as an isolated linguistic fact not immediately referable to known elements, in the hope that evidence acquired later, perhaps from an unlikely source, may set it in its historical perspective.

Such evidence appears to be presented by Hittite,[2] which has an

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[1] E. Boisacq, Dictionnaire étymologique de la langue grecque², 1923, s.v. ἀτύζω. Also, doubtfully, in Walde-Pokorny 2. 616.

[2] In accordance with Sturtevant's usage I write medial stopped (and affricative) consonants as single voiceless stops (-p-, -t-, -k-, -z-) if they regularly occur single in cuneiform syllabic structure, regardless of whether they appear as

adjective *ḫatuki-s* 'terrible, frightful' (abl. sing. *ḫa-tu-ga-ya-az*, nom. plur. *ḫa-tu-ga-e-eš*), an adverb *ḫatuka* (*ḫa-tu-ga*, *ḫa-du-ga*) 'terribly, frightfully' (interpreted by Sturtevant as a dative sing.), and a denominative verb in *-es-*, *-is-* (3d sing. present *ḫa-du-ki-iš-zi*) 'to become frightful', which are obviously close in both form and meaning to the Greek **atug-*.[3] That Hittite *ḫatuk-* and Greek **atug-* are not cognates independently derived from an Indo-European source is indicated by the Hittite lenis *-t-* (consistently written *-t-*, *-d-*, not *-tt-*, *-dd-*, as would be the case if the intervocalic dental stop were a fortis), which might be expected to correspond to a Greek *-d-* or *-th-* (IE *-d-* or *-dh-*), not to a Greek *-t-*. The facts are best suited to the hypothesis that Greek **atug-* or **atugi-* is an old Anatolian, presumably Hittite, loanword for 'terror' or 'terrible', lost as such but preserved in the denominative verb **atug-ye/o-* (**atugy-e/o-*) 'to become terrified'. The Hittite sequence of voiceless lenis stop : voiceless lenis stop ($t - k$) was heard by the Greeks as voiceless fortis stop : voiced lenis stop ($t - g$), precisely as in the case of Greek ὀπᾱδός: Hittite *ḫapatis* (Greek $p - d$ < Hittite $p - t$).[4] No doubt the loan word was a reference not to 'fear' or 'terror' in its generalized sense but to a more uncanny fear connected with violent movement or the overwhelming power of a god, perhaps a god manifesting himself in storm and thunder.[5] Culture loanwords are not as a rule mere substitutes or equivalents for commonplace words already in use. The reflex of such a storm-terror is perhaps still perceptible in the Homeric ἀτυζόμενος πεδίοιο 'coursing in terror over the plain'.[6]

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surds (*-p-*, *-t-*, *-k-*) or sonants (*-b-*, *-d-*, *-g-*) or both; and as doubled voiceless stops (*-pp-*, *-tt-*, *-kk-*, *-zz-*) if they are written double part or all of the time (whether as type *-pp-*, or type *-bb-*, or both).   The former set is probably to be interpreted as voiceless lenis, the latter as voiceless fortis.   The former set corresponds to the IE voiced and aspirated voiced stops (type *-b-* and type *-bh-*), the latter to the IE unvoiced stops (type *-p-*).   Initially only simple *p-*, *t-*, *k-*, *z-* are used for both type *p-* and type *b-* of Hittite, because it seems impossible to prove that the Hittite orthographic distinctions are phonetically significant.

[3] See Sturtevant, A Comparative Grammar of the Hittite Language (references given in Hittite index sub *ḫatukes-* and *ḫatukis*); also *ḫatugātar* 'terror' listed in his Hittite Glossary. See, for *ḫatuka*, also A. Götze and H. Pedersen, Muršilis Sprachlähmung, ein hethitischer Text (Det Kgl. Danske Videnskabernes Selskab., Historisk-filologiske Meddelelser 21. 4, 10, 17-20, 50.)

[4] See Sapir, LANGUAGE 10. 274–79 (1934).

[5] As the references in Götze and Pedersen's paper (1, 10) suggest.   See particularly 18 note 1.

[6] See Iliad, Z 38; Σ 7.   Terror in movement seems to be expressed in the other Homeric examples as well (Iliad: Δ 41; Θ 183; O 90; Φ 4, 554; X 474; particularly Odyssey: λ 606).

Can we go further and suggest cognates to Hit. *ḫatuk-* which would help us to fix an IE form with known type of dental? Pedersen remarks[7] : '-*ḫa-tu-* = gr. ὀδυ- 'in ὠδύσαο "du zürntest". Spuren eines dem heth. *g* entsprechenden Lautes sind im Griechischen anscheinend nicht vorhanden.' This etymology seems inacceptable for four reasons: (1) 'to be angry with' has only a subjective relation to 'terrible', which connotes fear, not anger; (2) it is not Gk. ὀδυ- which is comparable but ὀδυσ- (cf. ὀδυσ-θῆ-ναι; see Walde-Pokorny, 1. 174, sub 2. *od*); (3) **odus-* is probably a derivative of a basic **od-* 'to hate' (Lat. *od-iu-m*, *ōd-ī*, which is even further removed from 'terrible, terrifying'; (4) Gk. ἀ- of borrowed **ἀτυγ-* presupposes a type of postvelar spirant (*χ-*) in Hit. *ḫatuk-* which seems not to correspond to a cognate Gk. ὀ-.

Under *od-* 'Widerwille, Hass' in Walde-Pokorny are listed Anglo-Saxon *atol* and ON *atall* 'dirus, atrox'. This meaning again does not go well with 'to hate' or 'to be angry with', and we may surmise Germ. **at-la-z* < IE **χad-lo-s*, perhaps with non-syllabic -*w*- lost between consonants: **χadw-lo-s* 'terrible'. A Celtic cognate, possibly prototype of ON *atall* and AS *atol* if borrowed before change of IE *d* to Germ. *t*, would have to be **adwlo-*. The nom. sing. masc. **adwlos* would, in pre-Irish, have to take the form **ádwl̥* (with *a*-timbre of -*wl̥*), which, after spirantization of -*d*- to -*đ*-, change of -*w*- to -*b*- after -*đ*- (cf. *fedb* 'widow', i.e. *fedb* < **widh(ŭ)wā*),[8] and development of anaptyctic vowel (of *a*-timbre) + *l* from final syllabic *l*, should give **ádbal*, written *adbal*. Old Irish *adbal* 'valde, gewaltig' (not to be interpreted as *ad-bal*, i.e. *adbal* : Skr. *bála-m* 'power'[9]) is very likely the word we are looking for, in a specialized adverbial sense (probably < accus. neut. **adwlom* 'terribly').

If all this is correct, we can understand a parallel adjective in -*ro-*: **χadw-ro-s* 'terrible', preserved in Latin *āter* 'black' < 'sombre, unlucky, terrifying'.[10] If this is admitted, we at once get a better per-

[7] Götze and Pedersen, op. cit. 50.

[8] See Thurneysen, Handbuch des Alt-Irischen §199, a.

[9] See Walde-Pokorny 2. 111, sub 2. *bel-* 'stark.'

[10] See Ernout et Meillet's interesting comment in Dictionnaire étymologique de la langue latine, s.v. *āter*: 'Implique souvent une idée morale de terreur, de malheur, de mort, et ce caractère affectif de l'adj. explique qu'il soit particulièrement usité en poésie.' With regard to the *ātrī diēs* they quote from Gellius, ap. Macr.: 'pontifices ... statuisse postridie omnes Kalendas Nonas Idus atros dies habendos, ut hi dies neque proeliares neque puri neque comitiales essent', remarking of these 'black' (originally 'dreadful, ill-omened'?) days: 'Bien que l'origine en soit très contestée, l'expression provient peut-être de l'habitude de considérer comme des jours "noirs" ceux que viennent après les ides, c.-à-d. après

spective of its relation to *atr-ōx* (: *āter* like *acer-bus* : *ācer*, as noted in
Walde-Pokorny, Walde, and Ernout et Meillet).   Instead of explaining
atrōx as of 'dark mien' > 'frightful, terrible, cruel,' we would assume
that *atr-ōk-s* meant literally 'dreadful-faced' (cf. *fer-ōk-s*, *ferōx* 'wild-
eyed, -faced') and that *atr-* here preserves the older meaning which is
lost in *āter*.   Had *atrōx* been a simple derivative of *āter*, we would have
expected **ātrōx*.   The group IE *-dwr-* might readily develop to Latin
-tr-, to judge from the analogies suggested by: (1) *wr-* > *r-* (e.g. *rādīx* <
*wrād-); (2) *-dr-* > *-tr-* (e.g. *taeter* < **taid-ro-s* : *taed-et*).[11]   That *-w-*
would disappear in the group *-dwr-* is made as good as certain by *quad-
rāgintā* and *quadru-* < *-twr-*.[12]   The relation of *a* to *ā* in *atrōx* and *āter*
remains unclear but does not invalidate our construction.   It may be
suggested, in passing, that such cases as *atro-* : *ātro-* and *akro-* : *ākro-*
may go back to old intensive reduplications, e.g. **x̌adwro-* : **x̌ax̌dwro-*,[13]
which are analogous to such pairs as **kʷelo-* : **kʷekʷlo-*.

In view of Germ. **at(w)-la-* we must reconstruct Hittite *ḫatuk-* to IE
*x̌adug(h)-, not **x̌adhug(h)-*.   We are now even more certain that Greek
ἀτυγ- is borrowed from, not cognate to, Hittite *ḫatuk-*, and we see that
this stem is a specific Hittite derivative in *-k-* (IE *-g-* or *-gh-*) parallel
to derivatives in other dialects in *-l-* and *-r-*.   The genealogy of the
forms discussed in this paper may be represented in the following
scheme:

IE **x̌adw-* (**x̌adu-*) 'terror' >
    1. Hittite *ḫatu-* in derivative *ḫatu-k(ai)-* > (borrowed)
        a. Gk. **atug-ye/ₒ-*

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la pleine lune par opposition aux jours "clairs" de la lune croissante.'   (Cf. also
quīnqu-ātrūs, sex-ātrūs, septim-ātrūs '5, 6, 7 dreadful, unlucky [?] days following
the Ides', quoted from Wackernagel.)   But the original meaning of *āter* may have
been 'dreadful, inauspicious' with particular reference to 'darkening' storm
clouds, so that we are perhaps once more brought back to the storm connotation
noted by Götze for Hittite.   Aside from Umbrian *atru, adro* (neut. pl.) there are
no cognates of Latin *āter* which connect with 'black', though attempts have been
made to relate it to IE words referring to fire, hearth, burning, ash (see Walde-
Pokorny 1.42; A. Walde, Lateinisches Etymologisches Wörterbuch, s.v. *āter*;
Ernout et Meillet, op. cit., s.v. *āter*).

[11] See F. Sommer, Handbuch der Lateinischen Laut- und Formenlehre §127,
1; 3, b; R. G. Kent, The Sounds of Latin §181, V; §141, VIII.

[12] Sommer, op. cit. §138, 1.

[13] According to the de Saussure-Möller 'laryngeal' hypothesis, which is grad-
ually making its way in Indo-European phonologic theory and which seems
altogether inescapable to me, the *x̌* which colors a theoretical '*e*' to *a* is absorbed
into a preceding *a* < '*e*' with lengthening of this *a* to *ā* (full grade of one of three
types of 'heavy base' syllable).

2. *χadw-lo-
   a. Germ. *at-la-
   b. Celtic *adw-lo-
3. *χadw-ro-, reduplicated *χaχdw-ro-
   Italic *atro-, *ātro-

As we have seen, these words cannot be identified with IE *od-, *odus- 'to hate, hatred', as has been suggested for several of them. Gk. o- of ὀδυσ-, a high-grade vowel equivalent to normal e- (not o-ablauting with e-) is comparable with IE ǫ- of such words as *ǫpos[14] and *ǫk̂tó(u). The ǫ- of these words is due to the darkening effect of an originally preceding voiced velar spirant, γ-, which, like χ-, appears in Hittite orthography as ḫ- (e.g. ḫappar 'business transaction': Lat. opus, Toch. op(ä)ś-śi 'capable') but is probably to be read as a voiced spirant γ or its voiceless lenis correspondent.[15]   Now it so happens that we seem to have a reflex of IE *γǫdus- (*odus-, as ordinarily reconstructed) 'hatred' in Tocharian.   *γǫdus- must, for purposes of Tocharian phonology, be interpreted as *γǫdu̯s-, * γǫdwₑ-s-; i.e., IE 'u' and 'i' are not true vowels, phonemically speaking, for Tocharian but semivowels, which appear in Tocharian as y and yä, w and wä, respectively, Toch. ĭ and ŭ being merely timbred forms of IE ₑ (Toch. 'ä') or zero before original y and w or, for u, before and after labialized consonants.   Postvocalic -d- disappears in Tocharian.   IE *γǫdwₑs- (Gk. ὀδυσ-) should therefore appear as Toch. *owäs-.   Under certain circumstances, however—presumably when w is followed by a nasal—, w is nasalized to m.   A curious example of this rule is Toch. muk 'yoke', which looks like a perverted form of IE *yugóm but is probably a

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[14] I propose ǫ for this vowel (Brugmann's ᵭ) to distinguish it from the more common o, which ablauts with e.   Brugmann was misled by Armenian akn 'eye' into the belief that the high-grade ǫ was nearer to a than was the normal o.   Exactly the reverse is the case, as is shown by the fact that Toch. has o- for IE ǫ- (really γ ǫ-), e.g. okät and opäś-śi: Lat. octō and opus, while IE o- appears as Toch. a-, e.g. an-ac 'to him' < IE *ono-, Hittite ana-.

[15] There could be no other method of indicating this sound in cuneiform orthography; see Sapir, loc. cit.   For comparative purposes, and for comparative purposes only, it might be well to distinguish Hittite ḫ₁ (= IE χ) and Hittite ḫ₂ (= IE γ, at least in certain cases), leaving ḫ for ambiguous cases and for transliterations of cuneiform orthography.   We know from the reflexes of these two consonants in both Greek and Canaanite (Hebrew) that they must have differed in Hittite itself and these reflexes powerfully support the comparative Indo-European evidence.   Hittite ḫ₁ appears in Hebrew as ḥ (e.g. ḥittî : Hit. ḫatti-), while ḫ₂ appears in Hebrew as ' (e.g. tid'al): Hit. tudḫaliyas).   ḫ₁a-, i.e. χa-, appears in borrowed Greek words as ἁ-, as we have seen, while ḫ₂a-, i.e. γa-, appears as ὀ- (e.g. ὀπᾱδός: Hit. ḫapatis < Sem. 'abad-).

perfectly regular development of the analogical *yungóm (cf. Lat. iungere : iugum) which must also be assumed for Lith. jùngas. IE *yungóm must be interpreted as *y$_e$wngóm, reduced from *yew(n)g- (Gk. ζεύγ-νῡ-μι), actualized as pre-Toch. *y$_e$ẉgóm. In Tocharian -ẉ- (nasalized -w-) labializes and nasalizes y-, the resulting theoretical *ẉ$_e$w- appearing as mu-.[16] Similarly, *owäs-, appearing only compounded with -kem 'wrong',[17] becomes nasalized to omäs-; omäs-kem, a synonym compound of a type which is quite foreign to Indo-European and has obviously been due to Sinitic (Tibetan) influence, means 'hateful-wrong' > 'bad'.[18] Tocharian omäs-, in other words, is an almost unrecognizable reflex of an archaic Indo-European word which is best preserved in Greek as ὀδυσ-. There seems to be no other Indo-European language which preserves this derivative of *od- (*γọd-) 'to hate'. There is, however, a very common form of *γọdw$_e$s- with zero-grade of first syllable : *γdw$_e$s- > *dw$_e$s- (*dus-) 'badly'. This form is likely to have preserved the original meaning of the base *γọdwes-, in view of the meaning of Toch. omäs-.

[16] That Toch. m- < IE y- is no figment of the imagination here is proved by the cognate Toch. mokc- (in abl. mokc-äṣ) 'bowstring' < *yow(n)k-ter- (cf. Gk. ζευκ-τῆρ-ες 'straps attached to the yoke', Skr. yoktár-). -c- results from -t- palatalized by following -e-.

[17] See SSS 248.

[18] See SSS 221-8, for many examples of such synonym compounds. omäs-kem belongs to a specialized set of such compounds, in which the analysis is no longer obvious. A more transparent example is ñom-klyu (dialect B ñem-kälywe) 'fame' < 'name-fame' (IE* nōm$_e$n-k̂lewos); here too -klyu seems not to occur alone.

SOME HITTITE ETYMOLOGIES

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Friedrich[1] and Goetze[2] have recently shown that Hittite *ša-ak-nu-(wa-)an-za* means 'ritually unclean'. The word is used of persons who engage in ritual acts or enter a temple when they are ritually impure. A distinction is sometimes drawn between 'pure tables' (GIŠBANŠUR.ḪI.A *pár-ku-i-ya-aš*) and 'impure tables' (GIŠBANŠUR.ḪI.A *ša-ak-nu-wa-an-da-aš*), both of which are used in the ritual.

Goetze suggested that the adjective might be derived from the noun *ša-ak-kar* as *e-eš-ḫa-nu-wa-an-ta* (neut. pl.) 'bloody' is derived from *e-eš-ḫar* 'blood'. Since I have demonstrated[3] that the suffix *wanza* denotes possession, being cognate with Skt. *vant-*, etc., this amounts to interpreting *ša-ak-nu-wa-an-za* as 'having uncleanness'. The correctness of Goetze's suggestion is shown by KUB 10.60.1, where we read LÚ.MEŠ GIŠBANŠUR *ša-ak-na-aš* 'men of the table of uncleanness' instead of LÚ.MEŠ GIŠBANŠUR *ša-ak-nu-wa-an-da-aš* 'men of the unclean table'. The noun *ša-ak-kar* therefore means 'ritual impurity, uncleanness'.

Its primary meaning, however, is more concrete than that, as can be inferred from a passage from an unpublished text which Friedrich discussed, but which, I am sure, he failed to understand completely. Bo.10204.3.10–13: *ŠA* ᴰIM ᵁᴿᵁ*KU-LI-Ú-IŠ-NA* DINGIR LÚ.MEŠ *šu-me-eš e-iz-za-aš-tin* (11) *nu-za iš-pí-iš-tin e-ku-te-en-ma nu-za ni-ik-te-en* (12) *nu-uš-ma-aš* ŠA(G)-*KU-NU ša-ak-nu-an e-eš-tu* ZI-*KU-NU-ma-aš-ma-aš* (13) *wa-ar-ši-ya?·-an-za e-eš-tu*; 'Ye male gods of the storm god of Kuliwisna, eat and satisfy your hunger, drink and satisfy your thirst. Let your interior be full of *ša-ak-kar*, and let your spirit be calmed'. Friedrich thought that *ša-ak-nu-an* meant 'full', and he regarded it as a participial adjective, but now we recognize the noun from which it is derived. As far as I can see *ša-ak-kar* must here denote

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[1] Archiv Orientalní 6.365–8.

[2] Götze-Pedersen, Muršilis Sprachlähmung 35 and fn. 1.

[3] A Comparative Grammar of the Hittite Language 160 f. See also JAOS 54.405.

the products of digestion, namely 'excrement', or it must have its
secondary meaning 'ritual uncleanness'.   But the second alternative is
really impossible; the worshiper would not urge the gods to become
ritually unclean.   Therefore, strange as it appears to us, we must
suppose that he is explicitly asking the gods to incur the necessary
physiological results of eating a satisfying meal.

Confirmation comes from an obscure passage in KUB 7.5.1.7–10:
nu-wa-aš-ši-kán an-da-ki-it-ti-iš-ši (8) *kat-ta-an-ta pa-it nu-wa ka-a-aš*
ta-an-tu-ki-eš-na-aš DUMU-*aš* (9) *ša-ak-na-aš ši-e-ḫu-na-aš nu-ud-du-*
uš-ša-an Ú-UL (10) *ú-e-mi-ya-at*.   Goetze calls my attention to the
fact that this text is to be combined with KUB 9.27 and KUB 7.8, and
that the other two fragments (or possibly one should say the duplicate
text) show that we have before us a ritual for the cure of impotence.
Nevertheless a lacuna deprives us of the context needed for the inter-
pretation of the passage cited.   The preceding line contains the phrase
DUMU.SAL *su-up-pí-šar-aš*, which probably means 'virgin'[4] and which
is almost certainly the antecedent of -*ši* at the opening of our passage.
The dative *an-da-ki-it-ti* seems to come from a derivative in *t-, ta-,* or
ti-[5] of the verb *ki-* 'lie' with prefix *anda*; it may well mean 'intestines'
or the like.   There is nothing preserved to identify the subject of the
two verbs in the passage.   I translate tentatively: 'It went down into
her intestines(?).   And, behold! it found *tantukesnas* DUMU-*as saknas*
sehunas, but did not find you'.   I do not know the meaning or the case
of *tantukesnas*.   DUMU-*as* presumably means 'of a child'.   Both
ša-ak-na-aš and *ši-e-ḫu-na-aš* are genitives.   The only conclusions of
value for our present purpose that can be drawn are that *ša-ak-kar* is
again said to occur within the body, and that *ša-ak-kar* is paired with
še-e-ḫur[6] without a conjunction, which indicates a very close connection.
Hence, if *ša-ak-kar* is 'excrement', *še-e-ḫur* is 'urine'.

Goetze has kindly put at my disposal a passage from an unpublished
text, which enables us to test this hypothesis.   The text prescribes the
duties of the members of the guard of the palace, the LÚ.MEŠ *MEŠEDI*.
They are directed not to leave their posts; but (Bo. 2002.1.39–43):
ma-a-an-za-kán ga-ma-ar-šu-wa-an-za-an ku-in ta-ma-aš-zi nu a-ra-aš
a-ri te-iz-zi (40) *nu-uš-ša-an a-pa-at-za* A-NA GAL *ME-ŠE-DI a-ri*
še-e-ḫu-na-wa-ra-aš pa-iz-zi (41) *nu* GAL *ME-ŠE-DI te-iz-zi pa-id-*
du-wa-ra-aš LÚ *ME-ŠE-DI ku-iš še-ḫu-na pa-iz-zi* (42) ZI-*an-ša-an-za*

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[4] See Sturtevant and Bechtel, A Hittite Chrestomathy 108.1.25, 119 f.

[5] See HG 149, 157, LANG. 10.266–73.

[6] For this orthography, see KUB 22.33.1.9.

kap-pu-u-iz-zi nu-uš-ša-an še-e-ḫu-na-aš-ša ut-tar I-NA E.GAL-*LIM a-ri* (43) ZI-*it-ma-aš-kán pa-ra-a Ú-UL pa-iz-zi*; 'if anyone having a full bladder (??)[7] is distressed, he speaks to his comrade, and this (report) is brought to the chief of the guard: "He is going to urinate". And the chief of the guard says: "That member of the guard who is going to urinate may go". He takes account of his purpose, and word of the urination comes into the palace. But of his own accord (a member of the guard) does not go away (from his post)'. Evidently the word *še-e-ḫur* is here an action noun, as indeed its suffix (IE *wer/n-*) implies; but its secondary use as a concrete noun in KUB 7.5.1.9 is not strange.

The word *še-e-ḫur* must be taken in the concrete sense in the passage from an omen text cited by Goetze, NBr. 68, namely KUB 22.33.1.9: Ā[MUŠEN]-*ma* GUN-*an še-e-ḫur tar-na-aš* 'but shorewards(??)[8] the eagle emitted[9] urine'.

It appears to be certain, then, that *ša-ak-kar*, gen. *ša-ak-na-aš*, means 'excrement'; and, if so, it must be the same word as Gk. σκῶρ, gen. σκατός, both of which are typical *r/n-*stems. Goetze tells me that he thought of this etymology some time since, but he has never published it. It follows that Gk. σκῶρ is not a root noun from the base **sker-* 'cut' with analogical genitive, as suggested by Brugmann.[10] We may still connect OIsl. *skarn* 'excrement' and Skt. *kárīṣam* 'excrement', on the supposition that they are modifications or extensions of the old *r/n-*stem. We must apparently separate Skt. *apaskaras* and *avaskaras*, whose precise meaning and etymology is somewhat uncertain, since they clearly contain a verbal root ending in *r*. It is safer to exclude also Lat. *muscerda* 'mouse-dung'.

The etymology shows that the first vowel of the Hittite noun is merely an orthographic device to make possible the writing of the initial consonant group.[11] The doubling of a *k* which corresponds with IE *k* is quite in order.[12] Consequently *ša-ak-kar* is to be pronounced [sk·ar] and *ša-ak-na-aš* [sk·nas], if we assume that *kk* stands for a long consonant.

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[7] The hapax, *ga-ma-ar-šu-wa-an-za-an*, seems to be an adjective in agreement with *ku-in*. Possibly it contains the possessive suffix *want-* (with *za* instead of the usual *ta* or *da*). For the underlying stem, cf. the verb *ka-mar-ši-eš-kán-zi* (KUB 18.41.2.8), which stands in a mutilated context, but is followed in line nine by *wa-a-tar* 'water' and in line ten by *a-ar-aš-zi* 'flows'.

[8] See Götze, Kulturgeschichte 141.

[9] This meaning of *tarna-* is no doubt to be assumed also in passages from omen texts where a bird name is the subject and no object is expressed.

[10] Archiv für Lateinische Lexikographie 15.3 fn. 2, Grundriss 2².1.579.

[11] Sturtevant, HG 47 f.

[12] Sturtevant, HG 74–83.

Hittite has another word for 'excrement', namely *za-ak-kar*, which is defined in a vocabulary fragment (KBo. 1.45.1.9), and which occurs in the Instructions for Temple Officials,[13] where we are informed that if a kitchen worker causes the gods to eat from an unclean vessel the gods will give him dung and urine to eat and to drink' (*a-pí-e-da-ni-ma* DINGIR-MEŠ-*eš za-ak-kar* ⸗*du-ú-úr a-da-an-na a-ku-wa-an-na pí-an-zi*). Goetze has suggested (see fn. 2) that *za-ak-kar* and *ša-ak-kar* may be the same word, but that is unlikely, since the signs *za* and *ša* do not elsewhere alternate in Hittite. Furthermore *du-ú-úr* in the passage just cited in marked by the glossenkeil as a foreign word, and that fact suggests that *za-ak-kar* also may be foreign. Since the glossenkeil appears most frequently with Luwian words, one may be inclined to assign *du-ú-úr* to that language.[14]

In searching for an etymology for *sehur* 'urine', we are confronted with the difficulties presented by Hittite *h*. So far we have only one clear etymology of a Hittite word containing *h* between vowels, namely *pahhur* 'fire', which must be identified with Gk. πῦρ, etc. This word, to be sure, contains *hh* as against the single *h* of *sehur* and the preceding vowel is different also. None the less it furnishes the best clue we have as to what to look for in the historical IE languages.

This clue led me directly to OIsl. *sūrr* 'sour, unpleasant' beside *fūrr* 'fire'. The latter word represents a transfer from the *r/n*-declension to the *o*-declension, while *sūrr* is an *o*-stem adjective citable only from Germanic (OHG, OE *sūr* 'sour') and from Balto-Slavic (Lith. *súras* 'salty', ChSl. *syrъ* 'moist, raw'). If the suggested connection with Hittite *sehur* is correct, it is no doubt a derivative from the *r/n*-stem noun, and its primary meaning must have been 'like urine'.

Walde-Pokorny (2.513) hesitatingly connect with the adjective OIsl. *saurr* 'male semen, impurity, moist earth'. This word is semantically close to our Hittite noun. It may well be, like *fūrr*, an *o*-stem noun based upon an old *r/n*-stem. It seems to differ from *sūrr* in having IE *əu* instead of *ū*; but there are many parallels for that; e.g. Skt. *sthūrás* 'thick' : Gk. σταυρός 'prop'; Goth. *sūts* 'sweet' : Gk. ἀνδάνω (<*swəd-*) 'please'; Skt. nom. sg. fem. *-ī* : Gk. *-ĭă*. Both *sūrr* and *saurr* must differ in the ablaut grade of the radical syllable from Hittite

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[13] KUB 13.4.3.64–8 = Sturtevant and Bechtel, Chrest. 160.

[14] Is the glossenkeil omitted before *za-ak-kar* because it comes from another source? And may that source be Skt. *śakṛt* or its Indo-Iranian predecessor? There is no reason to suppose that Indic (or Indo-Iranian) loan words in Hittite were confined to names of gods and technical terms of horse training.

sehur, but that is quite in order in an *r/n*-stem; cf. Gk. ὕδωρ beside ἔαρ. Apparently Hittite *seh*- presents full grade of a heavy base and OIsl. *sa*- reduced grade of the same. Clearly all three words contain *ur*, reduced grade of the *wer/n* suffix.

The root in question can scarcely be other than IE *$s\bar{e}(i)$- 'throw off, scatter, let fall'. Hittite *seh*- may be equated with the monophthongal form of the IE base or it may contain the diphthong *ei* shortened before *h* and then monophthongized as the short diphthongs always are in Hittite. In other words it may be analogous to *nehhi* 'I lead, send, turn' $<$*neihi* $<$*$n\bar{e}ihi$.[15] OIsl. *saurr* must be connected with the monophongal form of the base. Possibly Gk. αἰονάω 'I moisten' is based upon a derivative in *wer/n* from the reduced grade of the diphthongal base (IE *səiwer/n*- beside *səwer/n*-). From the full grade of the monophongal form of the base comes Lat. *sēmen* 'seed', whose meaning 'seminal fluid', hitherto supposed to be a secondary meaning, is identical with one meaning of OIsl. *saurr*. In view of the well known equivalence and frequent interchange of the suffixes *wer/n* and *mer/n*,[16] *sēmen* is, then, practically the same word as Hittite *sehur*, and this fact may dispose us to see in the latter the monophthongal form of the base *$s\bar{e}(i)$-.

The connection of Hittite *sehur* with the IE base *sē*- adds plausibility to the connection of Hittite *mehur* 'time, point of time', with the IE base *mē*- 'measure', which I once suggested with some hesitation.[17] Conversely, since *mehur* must go with the monosyllabic base *mē*-, it becomes still more probable that *sehur* belongs to the monophthongal base *sē*-.

So far I have discussed etymology from the point of view of the generally accepted theory of Indo-European ablaut. But there is another theory, advanced long ago by de Saussure, and recently elaborated by Kurylowicz,[18] according to which the long vowels of the so-called heavy bases are really due to the loss of consonants that followed an originally short vowel. Kurylowicz designates these consonants as $ə_1$, which converted preceding *e* to *ē*; $ə_2$ and $ə_4$, which con-

[15] See Sturtevant, LANG. 7. 115 f.; HG 110.

[16] For the origin of the variation in the initial consonant, see Sturtevant, AJP 50. 360–9; TAPA 60. 33 f.; LANG. 7. 170; HG 114–6.

[17] Revue Hittite et Asianique 1. 78; LANG. 7. 119. The inclusion of Hittite *mahhan* proves to have been an error; intervocalic *h* and *hh* are different phonemes.

[18] See most recently Kurylowicz, Études Indo-Européennes 1. 27–76 (Cracow, 1935).

verted preceding e to $\bar{a}$ and following e to a; and ə₃, which converted preceding e to $\bar{o}$ and following e to o. Sapir has suggested, in a paper read before the Yale Linguistic Club, that we should consider Kurylowicz's ə₁ a glottal stop and ə₂ a voiceless velar spirant (the other two consonants do not concern us at present). It is assumed both by Kurylowicz and by Sapir that ə₂ $= x$ (voiceless velar spirant) appears in Hittite as h, and a corollary of the assumption is that any vowel before or after Hittite $h = x$ must represent an inherited a or its reduction. Since original a remains in Hittite it should be easy to test the hypothesis; the only known phonetic changes that would obscure the facts are the change of diphthongs with short prior elements into e (often written i) or u. If one bears these changes in mind he will find that a very large majority of the Hittite words containing h conform to the requirements of the theory.

There remain, however, some striking exceptions, and these have hitherto prevented me from accepting the doctrine. Typical instances are the words *sehur* and *mehur*, which contain e before h. We now see that this vowel corresponds to IE $\bar{e}$ (of the traditional system), and that means, in Sapir's system, e'. Now it is noteworthy that Hittite h from the voiceless velar spirant is regularly written double when it stands between vowels, as for example in *pa-aḫ-ḫur* 'fire'. In *sehur* and *mehur*, however, h is always written single. I can cite one other common word in which intervocalic h after e is consistently written single, namely *weh-* 'turn' (pret. 1 sg. *ú-e-ḫu-un*, midd. pres. 3 sg. *ú-e-ḫa-at-ta*, *ú-e-ḫa-at-ta-ri*, pl. *ú-e-ḫa-an-ta-ri*, *ú-e-ḫa-an-da-ri*). The IE languages contain numerous words meaning 'turn' with initial *we-*, but I cannot identify any of them with Hittite *weh-*. I can make no advance beyond my suggestion of eight years ago[19] that Hittite h is here a root-determinative parallel to r, l, and i in the IE languages. Nevertheless Hittite itself shows that in *weh-*, as in *sehur* and *mehur*, a single intervocalic h leaves a preceding e unchanged in quality.

These three words suggest that an original glottal stop between vowels appears in Hittite as a single h. This conclusion is confirmed by the fact that in the texts from Boghazköi Akkadian words are sometimes written with $ḫ$ (usually but not always the sign is *aḫ*). René Labot[20] gives citations from the texts written entirely in Akkadian.

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[19] LANG. 4. 161. It is probably a coincidence that a majority of the words for 'turn' in the early IE languages begin with *wer-*, and that in Hittite texts *weh-* 'turn' is sometimes confused with *wer-* 'burn' (see Götze-Pedersen, Muršilis Sprachlähmung 28–32).

[20] L'Akkadien de Boghazköi 32 (Bordeaux, 1932).

There are other occurrences in Akkadian words included in Hittite texts. It is, therefore, at least possible that the intervocalic *h* in our three Hittite words is a notation for a glottal stop that had been retained without change.

All this, of course, has no bearing upon the assumption of loss of the glottal stop in other positions. Both Kurylowicz and Sapir assume that the sound was lost in Hittite, and it seems to me that this is almost certainly the case initially and before consonants.

MISCELLANEA

THE MEANING AND ETYMOLOGY OF HITTITE *salik-*

Paragraph 195 of the Hittite Law Code has in part usually been trans-
lated about as follows: 'If a man has a free (woman) as wife and lies
with(? *salika*) her daughter also, (there is) the death penalty. If he
has the daughter and lies with(?) her mother or her sister, (there is)
the death penalty.' There is no other evidence that the word *salika*
means 'lies with', but this passage seemed to prove that meaning approxi-
mately correct. Consequently I proposed[1] several years ago an ety-
mological connection with Gk. λέχος 'bed', Homeric λέκτο, ἐλέξατο
'lay', etc., regarding the initial syllable *sa* as the Hittite development of
original *sm-* 'together, with'. There is no phonological difficulty with
the etymology. There are several parallels for the appearance of IE
syllabic *m* as Hittite *a*.[2] The written *i* of the second syllable may stand
for spoken *e*.[3] The *o*-grade of the uncompounded verb appears in Hitt.
laki (*la-a-ki*) 'causes to lie, causes to fall', midd. *lagari* (*la-ga-a-ri*) 'lies,
falls'.

Friedrich[4] has recently discussed all occurrences of the stem *salik-*
in the published material from Boghazköi, and when I first read the
article I thought that he had completely disproved the meaning 'lie
with' and consequently my etymology of the word. He points out that
Hittite ordinarily expresses the meaning 'lies with' by *katta seszi* in
case no blame attaches, while incest and intercourse with animals call
for the phrase *katta wastai* 'sins with'. In all occurrences of *salik-*,
except paragraph 195 of the Code, the context, he thinks, demands some
such meaning as 'be impure' or, if the verb is accompanied by a dative,
'cause impurity in, defile'.

Friedrich finds the primary physical meaning of the verb in KUB
13.4.3.64–8,[5] which runs as follows: 'If you (i.e. workers in the temple

[1] LANG. 6.216 f.
[2] See my Hittite Grammar 105.
[3] See HG 50 f. and fn. 39.
[4] Archiv Orientální 6.358–65.
[5] This text I have published in transcription and translation in JAOS 54.363–406
and again with a cuneiform text in Hittite Chrestomathy 127–74.

kitchens) have any implements of wood or (any) implements of asphalt, and if then a pig (or) a dog ever causes uncleanness (*salika*) among (them), and the kitchen worker does not throw them away, but causes the gods to eat from an unclean (vessel), then the gods will give him dung (and) urine to eat and to drink.' Friedrich argues from the penalty prescribed that the pig or dog must have voided excrement in the dishes. It scarcely need be said that the conclusion is unnecessary; it is at least equally satisfactory to suppose that the unclean animal defiles the temple utensils by mere contact. In other passages Friedrich himself recognizes that this is all the verb means; as in 3.78–80 of the same text: 'But if he purposely postpones (a ritual bath), and, (when) he has not bathed, he defiles (*salika*) the ordinary bread and the libation bowl of the gods (by being) unclean near (them), . . . '

Sommer informs me by letter that he considers the primary meaning of the verb stem *salik-* something like 'an etwas stossen, berühren', and there is no doubt that such an idea would fit in all the passages discussed by Friedrich, except in two or three that still defy interpretation altogether.

Now, it is evident that the meaning 'touch' is near to the meaning 'lie with, lie together', and the phonological argument in favor of the etymology which I formerly suggested is strong enough to deserve some consideration. I therefore propose to retain in paragraph 195 of the Code the translation of *salika* that was given above. The idea of defilment was no doubt inseparable from the word even at the early date when the Code was composed, as we may infer from the restriction of the word to a context which implies blame; but we may safely leave that implication to the context of our translation. In the passage from KUB 13.4.3.64–8, I should now translate *anda salika* 'lies among (them)', although in 3.80 of the same text, where *salika* has a personal subject and no verbal prefix, but an accompanying dative, it is necessary to translate 'defile'.

In the forthcoming second edition of my Hittite Glossary, I propose to define *salik-* thus: 'lie together, be contaminated'; with the dative 'lie with, contaminate, defile'.

E. H. STURTEVANT

SEMANTIC NOTES TO LATIN *swerō 'SPEAK'

Existence of an Italic *swero 'speak', 'swear', 'declare on oath' may be assumed from Latin *sermo* from *swermō,[a] O *sverunnei* 'to the speaker',

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[a] [For the phonology cf. Walde, Gesch. d. idg. Sprachw. 2.1.172; others would assume a base with doublet form *s(w)er-.   GMB]

and from other survivals, which have been overlooked, chiefly because of semantic contamination with *sero* 'join'.   For example, the phrase *in libertatem adserere*, although listed in the lexicons under *adsero* 'join to', is better explained as equivalent to *adesse apud praetorem et iurare aliquem libertate dignum esse*.   A similar interpretation is to be assumed in the contrary formula *in servitutem adserere*.   Hence from **adswerō*. The association of this verb with legal proceedings is proethnic: OHG *sweren, swerien*, AS *swerian*, NE *swear*.

The case is similar with *desero*.   If from *sero* 'join' we should expect it to be construed like *disiungo*, with *de* or *a* and an ablative, but the prevailing use is absolute or transitive.   This points to **deswerō* in the sense 'forswear'.   Association with soldiers, litigants and lovers, all under oath, confirms this identification.   The phrase *loca deserta* will then mean 'regions forsworn, accursed, uninhabited'.

Sors 'oracle', 'lot' is usually ascribed to *sero* 'arrange in order', but disorder and not order is characteristic of lotteries; besides, this explanation fails completely to account for *sors* 'principal of a loan'.   Therefore the primitive is rather **swr̥tis* than **sr̥tis*, *w* being regularly lost after *s*.[b] *Sors* is then the 'word' of the borrower, his verbal promise to pay, and *sortes Fauni* means literally the 'utterances' of the god, just as Cicero and Vergil understood it.[1]   The drawing of lots was of course to ascertain the 'voice' or 'will' of a god.   With *sors* 'promise' may be compared *fides* in the sense of 'promise', which also derives from a verb of 'saying': IE *bhedh-*, OHG *bittan, bitten*, NE *bid*. *Consors*, again, through contaminated with *sors* 'lot', means properly 'one who assumes an obligation jointly with another', such as a wife or a partner in business.

There was also semantic contamination of *dissero* 'discuss' with *sero* 'join', but grammatical usage points to the true explanation.   If from *sero* 'join' we should expect it to be construed like *discepto, discutio, disputo*, but usage calls for *de* and the ablative when not used absolutely. This points to **dis-swerō* 'discourse upon' as the primitive.   Into this connection semantic considerations bring *disertus* 'eloquent', 'gifted with speech', but the short *ĭ* with the single *s* remains to be explained.   If we compare *dispicio* for *dis-spicio* it is assumable that one *s* disappeared while the *w* was still pronounced; then the *w* disappeared, leaving the *ĭ*,

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[b] [The statement seems too sweeping; but phonologically *sors* can come from **swr̥tis*.   GMB]

[1] De N.D. 2.2.6; 3.6.15; Aen. 7.81,102,254, where *oracula, responsa*, and *sors* refer to the same thing.

thus: *dis-swertos > *diswertos > disertus.[c]   On the same principle we
should expect dīsero, but the verb has obviously been rebuilt on the false
assumption of a connection with sero 'join'.   It may be added that dis-
in *dis-swertos was intensive, 'very eloquent', a force derived from
*dwis in the sense 'twice'.   This monosyllable suffered a split semantic
development: (1) 'twice', a force which attached to the later form bis;
(2) 'in two directions', 'apart', but *dwis must have at one time carried
both meanings.

Semantic considerations demand that adsevēro and persevēro be con-
nected with *swerō, since they mean respectively 'assert emphatically'
and 'persist in asserting'.   Moreover, since they are denominatives of
sevērus, it follows that this adjective may be interpreted as a dissyllabic
form of the root swer-.   The biblical 'I have sworn and I will perform it'[2]
describes the meaning precisely.   The vir sevērus is a man 'of his word'.
For example, Manlius Torquatus issued an order; for disobeying it he
put his own son to death.[3]   Other accounts of sevērus, which assume se-
to be privative, may be found in Walde,[4] but Ernout and Meillet
reasonably reject them all, though without offering an alternative.[5]
The above explanation is offered as a suggestion.   While it meets
semantic requirements neatly, to find a parallel for the vowel grades is
difficult: sincērus and prōcērus seem to be compounds; canōrus, sonōrus,
decōrus exhibit a similar pattern of short and long, but the r belongs
to the ending.

It is likely that servus should also be ascribed to this root.   *Swerwos
would mean 'gifted with speech', 'human', as distinguishing the slave
from other chattels.   Varro classifies chattels as follows: genus vocale
et semivocale et mutum: vocale in quo sunt servi, semivocale in quo
sunt boves, mutum in quo sunt plaustra.[6]   This suggestion is in accord
with the apparent derivation of famulus from for 'speak'.   This in turn
is consistent with the use of familia to denote all the human members
of the household taken collectively, both slave and free.

                                               NORMAN W. DEWITT

[c] [Kent suggests a better explanation: *dissértos > disertus by the law of
mamilla; it is so detached in meaning from dissero that an analogic *dissertus
is not to be expected.   GMB]
  [2] Psalm 119.106.
  [3] Livy 8.6–7.
  [4] Lateinisches etymologisches Wörterbuch²; Heidelberg, Carl Winter, 1910.
  [5] Dictionnaire étymologique de la langue latine; Paris, Klincksieck, 1932.
  [6] De R.R.1.17.1.

THE PRONUNCIATION OF *siegat* IN THE OLD SPANISH GLOSSES OF SILOS

In discussing the forms *siegat* (from Lat. *sedeat* > VL **seyat* > OSp. *seya* > mod. Sp. *sea*), *siegan*, *siegant*, and *siegam* found in the Old Spanish Glosses of Silos,[1] Priebsch attributes the presence of the *g* to the analogical influence of *diga* and *faga*. Menéndez Pidal, on the other hand, in publishing both the text and the glosses[2] considers the *g* an orthographic variant of *y*,[3] and maintains, therefore, that *siegat* is a scribal variant of *sieya*.[4] As for myself, I am inclined to believe that Priebsch was correct in his contention that the *g* (a voiced velar continuant) is due to analogical influences. The reasons that move me to this conclusion are as follows:

(1) Altho *siegat*, etc., appears in other medieval MSS, I have selected the text and glosses as published by Menéndez Pidal as the basis for my discussion because in the opinion of Father Villada the text and glosses are not only contemporaneous but also written, perhaps, by the same hand.[5]

Menéndez Pidal, in considering the *g* an orthographic variant of *y*, bases his conclusion on the fact that there have been found instances in medieval orthography of a *g* with the value of *j* or *y*, even before the vowels *a, o, u*.[6] This fact, of course, is entirely true, but I doubt very seriously that it applies to *siegat*. An examination of the MS would not disclose, I think, a single instance in which *g* is used either in the text or the glosses with the value of *j* or *y* before the vowels *a, o, u*. Since the scribe uses such consistency in writing the present subjunctive of OSp. *seer* or *ser* with a *g*, one would naturally expect a *g* in Lat. *habeat* > VL **hajat* > old and mod. Sp. *(h)aya*. But the form found in the glosses is *ajat* in all cases, the *j* having the value of *y*. Therefore, in the light of the orthographic style employed by the scribe in using only a *g* before the vowels *a, o, u* when he desired to convey the natural value of that consonant before these vowels, his marked consistency in availing himself of a *g* in writing *siegat* (8 times), *siegan* (5 times), *siegant* (3 times), and *siegam* (3 times), must lead to the conclusion that the scribe desired to express the sound of a voiced velar continuant.

[1] Altspanische Glossen, Zeitschr. f. rom. Phil. 29. 16–17 (1895).
[2] Orígenes del español 10–27 (Madrid, 1926).
[3] Ib. 375 note.
[4] The discussion in so far as it concerns *siegat*, etc., applies with equal force to the forms *sega*, etc., found in MSS of San Millán (Logroño), Sobrarbe, Sahagún, León, etc.
[5] Orígenes 11.
[6] Orígenes 53.

(2) In regard to the contention that the *g* is analogical: I believe that we have an abundance of verbal phenomena both in OSp. and present-day dialects to suggest a strong likelihood of an analogical influence being brought to bear on *seya* so that it became *siegat* or *sega*. Thus we find VL *cadeam*, etc., > OSp. and dialectal *caya*, etc., supplanted by mod. Sp. *caiga*, etc., in which the *g* is analogical; Let. *audiam*, etc., > OSp. and dialectal *oya*, etc., as opposed to mod. Sp. *oiga*, etc. So strong is this analogical influence of *g* that among some writers of the classic period we find *haya* > *haiga*, *vaya* > *vaiga*, *huya* > *huiga*, *destruyo* > *destruigo*, *restituyo* > *restituigo*.[7] The first three forms are still found in popular speech, and in addition we find, as in Bogotá, *crea* > *creiga*, *lea* > *leiga*, *ría* > *reiga*, or in New Mexico *vea* > *veiga*.[8]

(3) It would seem hardly possible that OSp. *seya* managed to escape this analogical influence. That it did not is fully evidenced, I think, not only by the forms appearing in the Glosses of Silos and elsewhere, but also by the living testimony as offered by the Monastir dialect of Judeo-Spanish. This dialect, I may say in passing, is perhaps the most archaic of all the Judeo-Spanish dialects spoken today. While preparing the study[9] of this dialect in Monastir, Yugo-Slavia, I discovered that the present subjunctive of *ser* was as follows:

seye	*si̯amus*	or *si̯emus*
seyes	*si̯áš*	or *si̯éš*
seye	*seyen*	

seġe	*siġamus*
seġes	*siġáš*
seġe	*seġen*

It is pertinent to remark here that the latter forms are but rarely used, and may in time disappear. The reason is largely psychological. The antiquity of the dialect and its unusual forms often become the object of ridicule on the part of those who speak the more popular dialects, of Salonica and Constantinople for example. Rather than

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[7] R. Menéndez Pidal, Manual de gramática histórica española §113.2b (Madrid, 1914). *Haiga* and *vaiga* have a wide dialectal acceptance as evidenced by their presence in the dialects of Salamanca, León, Bogotá, New Mexico, Costa Rica, Puerto Rico, and of Judeo-Spanish.

[8] A. M. Espinosa, Estudios sobre el español de Nuevo Méjico §195 (Buenos Aires, 1930).

[9] A study of the Monastir dialect of Judeo-Spanish based on oral material collected in Monastir, Yugo-Slavia, Rev. Hisp. 79. 323–583 (1930).

suffer this ridicule, the younger generation of Monastir is adapting its speech to that of the more populous and progressive center of Salonica.

The -$\dot{g}$- as employed in my transcription is pronounced very much like the mod. Greek γ or the Arabic $\dot{\dot{\dot{\text{ع}}}}$ (ghain). It corresponds, however, to the Spanish voiced velar continuant. Final unaccented Spanish *a* or an unaccented *a* followed by an *s* becomes *e* in the dialect. Thus reconstructing our Monastir sounds in terms of their corresponding sounds in Spanish, we have *sega*, or the solution to the problem of the pronunciation of *g* in *siegat*, *sega*, etc.

<div align="right">Max A. Luria</div>

BOOK REVIEWS

The Psycho-Biology of Language. Pp. ix + 336 and 7 plates. By
GEORGE K. ZIPF. Boston: Houghton Mifflin Co., 1935.

This is the third publication by Zipf on the theory and application of
his principle of relative frequency in the structure and development of
language. His first treatise on the subject[1] applied the theory to accent
and phonology, and laid the foundation which he still uses with incon-
siderable modifications. It was reviewed, in a generally sympathetic
spirit but with regretful refusal to agree that the problems attacked
had been solved, by Elise Richter[2] and by Eduard Hermann.[3] The
second book was shorter.[4] In it Zipf presented his study of the vocab-
ulary of four Plautine plays, and a phonetic, syllabic, and vocabulary
study of 20,000 syllables of connected text in the colloquial Chinese of
Peiping; there were hints of a contemplated extension into the field of
morphology. It was reviewed by Eduard Prokosch[5] with a severity
that bespoke a painful disappointment of his hopes for the possibilities
of the new line of inquiry. In the present volume Zipf embraces the
whole range of linguistic study and phenomena, from phonemes to 'the
stream of speech and its relation to the totality of behavior'. Ap-
parently nothing remains untouched within that range, and the treat-
ment almost uniformly evidences a belief that the author has attained
valid formulations. Further, the book is subtitled 'An Introduction to

[1] Relative Frequency as a Determinant of Phonetic Change, Harvard Stud.
Class. Phil. 40.1–95 (1929).

[2] ASNS 157.291–6 (1930).

[3] PhW 51.598–603 (1931). Other reviews: Kent, LANG. 6.86–8, 'not convinced
. . . despite the statistics'; Meillet, BSL 31. 3.17 (1931) (not available, but
Meriggi, IdgJb 16, reports 'ablehnend'); Meriggi, IF 50.246–7 (1932), 'calls atten-
tion to neglected factor' but 'exaggerated, almost mechanical utilization' and
'paper phonetics'; Sütterlin, LGRPh 52.241–3 (1931), appreciative; Twaddell,
Monatshefte f. deut. Unterricht 21. 230–7 (1929), appreciative and constructively
critical.

[4] Selected Studies of the Principle of Relative Frequency in Language (Harvard
Univ. Press 1932) viii + 51 and plates 62.

[5] LANG. 9.89–92. Other reviews: Cohen, BSL 33.3.10 f (1932) (not available);
Malone, MLN 48. 394 f. (1933) 'deserves credit for taking the first steps' but
'does not seem to realize that his task has just begun.'

Dynamic Philology', and to judge from the text this means a comprehensive survey of an established science written by an adept. We may therefore take the book for a complete though perhaps not the definitive presentation of Zipf's doctrine, and consequently believe that this is a proper time and occasion to attempt a critique of that doctrine, of its substantiation, and of its application.

The thesis, very briefly stated, is that the key to the explanation of all synchronic and diachronic language-phenomena has been found in a statistically established tendency to maintain equilibrium between size and frequency.[5a] Previous critics found the conclusions rash and largely improbable; they placed the blame partly on the introduction of a new technique into linguistic study. If they conceived an unjustly harsh opinion of statistical method in linguistics, the mistake was a natural one, for there was no one to warn them where statistics left off and explanation began except Zipf himself. As the matter now stands, neither the usefulness of statistical method in linguistics nor the value of Zipf's daring and ingenious explanations can be properly appraised, for they have not yet been separated. The separation and the separate appraisals will be the subject of this paper.

Before proceeding with the critique, it is proper to issue a general warning. The statistician avoids the popular concept 'cause and effect' and prefers to work with the concept 'functional interrelation' as it is used in mathematics and natural science, where the word 'function' has a technical meaning. When the statistician is confronted with two variable quantities in a complex of phenomena, he sets himself to observing whether certain values of variable A are associated (in his observation) with the probabilities or possibilities (both a posteriori) that variable B will have certain of its possible values. The two are said to stand in 'functional interrelation' when every possible choice of a value of A is found associated with a restriction of the possible or probable values of B. Two particular cases will be of interest: (1) Each variable is said to be a single-valued 'function' of the other when

[5a] In greater detail: (1) That relatively frequent use of a linguistic unit causes it to be reduced in one or more of its various kinds of magnitude—accent, complexity of articulation, extent in time, number of components, etc.—while relative infrequency of use occasions corresponding enlargements; (2) that this Law of Abbreviation has been established by statistical study; (3) that this Law can serve as the basis of a new science of language; (4) that current techniques of linguistic science thereby become partly obsolete, partly ancillary. These formulations are my own; the corresponding statements and implications in Zipf's writings are scattered and diffused throughout his publications.

the restriction is always to a single value of B, so that a choice among the values of A implies a choice of a certain value of B.   (2) The two variables are said to stand in 'statistical correlation' when it is a restriction of the probable values of B, so that the probabilities that B has certain of its possible values are different from what they were before the choice was made.   There is a full range of possibilities in strictness of correlation.   Evidently a single-valued function is that limit case of correlation which could be called 'perfect'; it is indeed that case which is marked with the statement 'correlation equals one', and the 'correlation' (η) used here is so defined that it can be calculated from numerical data. There has been observed a correlation between the heights of husbands and wives; it is measured (in the United States) by $\eta^2 = 0.20$ approximately, which means that when you meet a stranger in a well-lighted place you have a 12 percent better chance $\left(\sqrt{\dfrac{1}{1-0.20}} = 1.12 \right)$ of guessing his wife's height than if it were too dark to judge his height.   But it does not mean—and does not say— that a man's height partly determines his wife's height!

It should be particularly noted that here the variables are called 'A' and 'B' instead of being conventionally marked 'independent' and 'dependent'.   Mathematicians know that the employment of the latter terms is arbitrary: either variable may be called 'dependent', for each is by definition equally a function of the other.   By avoiding that arbitrariness we are enabled to see the beauty of the technique: we see that talking about functional interrelation does not imply a judgment as to which is the cause, or even a judgment as to whether or not there is any such thing as cause and effect.

On Plates I–III (44) Zipf graphically presents his data on the frequency-distribution of words in Chinese, English, and Latin.   Having done some work of this sort myself, and having seen a much larger amount done by another man, I am in a position to judge the validity of this: the data are adequate and are correctly represented by the points plotted.   The variables I shall call f (the FREQUENCY or times that the same word occurs in the text studied) and n (the NUMBER of different words which have the same frequency).   A glance at the charts suffices to show that there is unquestionably a statistical correlation between n and f, and that apparently it is close to that sort of functional relation which would be represented by a straight line on the chart (namely $nf^a = k$ where k is a constant); further, rough measurement shows that in all three languages the value of a is about 2.   For

Zipf this nearness of a to 2 is a discovery of cosmic import: 'But the overwhelming disclosure is this, that the formula for abbreviation is $ab^2 = k$, a formula exactly identical to that of gravitation.'[6] And so he is ready to disregard the possibly significant differences among the three languages, and to find that in all three a equals exactly 2; I quote, replacing his symbols with mine, from page 41 of the present book: 'Now, the line drawn approximately through the center of the points in each chart represents in each case the formula $nf^2 = k$.' I do not know how Zipf drew his lines; if by eye-measure, then he has excellent eyes, but not good enough to settle a point of such great theoretical importance. Taking the first twenty points for Chinese and the first thirty for Latin (first from above in the tables (26–7), first from below on the charts), which are all the points we dare use for reasons which Zipf properly mentions (43), and applying the laborious but exact method of least-squares, we find that for Latin the best straight line is $nf^{1.988} = k$ and that for Chinese it is $nf^{1.93} = k$. The lines are good fits for the points chosen.[7] It should be noted that a mathematically complete set of points would include not only the extreme points from the data (e.g., $n = 1, f = 514$ in Latin) which lie outside the charts on the upward extension of the left margin, but also points for all the gaps in the series of possible values of f (e.g., $n = 0, f = 60$ in Latin), which points all lie at an infinite distance to the left; since the latter lie below the line while the former lie above it, there is no justification for the statement (42) that 'If one extended the diagonal line on each chart to include these words of great frequency, the line would bend up sharply.' For Latin indeed, the only proper interpretation of Plate IV indicates that the line on Plate III, if extended to the left, would bend DOWN, AND NOT AT ALL SHARPLY. The English line on Plate II would remain fairly straight; the Chinese line on Plate I would bend like the Latin one (cf. footnote 9). Incidentally, the line which Zipf drew on the Chinese chart is about $nf^{1.97} = k$, so that it lies half-way between its true place and the place where he says it is; or, to put it mildly, his line forms a connecting link between data and theory.

In order to determine the significance of the index and of its nearness to the number 2, we must study the related chart on Plate IV. There we find words ranked in order of frequency. I shall use the symbol r for the RANK of a word, assigning $r = 1$ to the most frequent word. We find, in agreement with Zipf, a close approximation to the functional interrelation $fr = k$; that is, the second-most-frequent word

[6] Zipf, Selected Studies 24.

[7] The fit on Plate I is measured by $\eta^2 = 0.974$. That is, it is 97.4% perfect.

is about half as frequent as the first, the third in order is one-third as frequent as the first, and so on.   Now the sum of all the individual word-frequencies must be the LENGTH (L) of the text, and the sum of the relative frequencies $\frac{f}{L}$ equals *one*.   In English the relation between rank and frequency is $\frac{f}{L} = \frac{1}{10r}$ approximately, and we have $\frac{1}{10} \sum \frac{1}{r} = 1$, where the summation extends, word by word, through the whole vocabulary of the text.   But $1 + \frac{1}{2} + \frac{1}{3} + \frac{1}{4} + \cdots$ is a divergent series, and only about 12,000 terms are needed to give the required sum *10*. Yet the relative frequency of the most frequent word *the* ought to be the same for any amount of text up to the whole contents of all the English books ever printed and beyond.   And our dictionaries recognize over a quarter of a million words instead of the indicated 12,000.   It follows that the relation $fr = k$ can hold only for vocabularies of the order of magnitude found in Zipf's samples, and that Dewey's count[8] of 100,000 words of connected texts, with its 10,161 different words, already approaches the limit.   Let us therefore replace the divergent series by a convergent series on the assumption (which I shall continue to treat as an assumption) that the lines on Plates I–III must be straight lines and not curves.   On that basis the only possible series is $\frac{1}{10} \sum \frac{1}{r^{1+b}}$, where b is a small positive fraction, and then we can have a vocabulary of any size we please, each size corresponding to a particular value of b. An infinite vocabulary corresponds to b a trifle more than *0.10* (since $b \int_1^\infty \frac{dr}{r^{1+b}} = 1$ $[b > 0]$ where the small error made in replacing the series with this continuous function can be closely estimated).   If the relation $nf^a = k$ holds, there is a definite correspondence between vocabulary-size and the value of b, and it is: Infinite vocabulary: $b = 0.106$; very large vocabulary (English dictionaries): $b = 0.08$ approximately; about 12,000 words: $b = 0$; still smaller vocabulary: b negative (divergent series).   And b cannot be greater than about *0.106*.   Now since, by definition of n, r, and f, we can say that $n = -\frac{\Delta r}{\Delta f}$ when $\Delta f = 1$, we can also, with trifling error, say that $n = -\frac{dr}{df}$.   Therefore the same b

[8] Godfrey Dewey, Relative Frequency of English Speech Sounds (Harvard Univ. Press 1923).

reappears in the formulas for the lines on Plates I–III, and their indices are not 2 but $1 + \dfrac{1}{1 + b}$. If we replace this with $2 - c$, then c will be nearly equal to b when b is small, and will always be less than b. If the line is straight its index cannot be less than 1.90.

The reason for this brief excursion into the infinitesimal calculus is of course to get the argument on the record for any competent person to check; if any further excuse is needed, let it be the fact that Karl Verner could have checked it himself. Coming back to Plates I–III, we are now able to say that, if a straight line is a good representation of the relation between n and f, then a will have to be very close to 2 in its formula $nf^a = k$. It will be more than 2 for very small vocabularies, less than 2 for large ones,[9] and cannot be less than 1.90. If the 'best' straight line has an index less than 1.90, then it is not as good a fit as some curve would be. A sinuous curve with curvature AT EACH END would counterfeit what we have just found true of straight lines, but that is not what we find on Plates I–III (cf. footnote 7). This closes the use of the explicit assumption made above, and leaves the account balanced.

We can now leave Zipf's 'overwhelming disclosure' and turn to an error which crept in because of it—because he took 2 for a sort of ideal value for the index and correspondingly took the straightness of the line for granted. If the index of the best straight line is less than 1.90, then a better fit would be a curve that is concave below, as can easily be demonstrated. That is the situation on Plates VI–VII (256), where Zipf nevertheless drew straight lines. The indices of the best straight lines for those charts would have only the one virtue of measuring a sort of average slope of the proper curves. To draw straight lines on those charts, and consider them as any sort of representation of the data, is an absurdity of which no statistician would be guilty—he would recognize it as an absurdity inherent in the fact that the definitions of n and f exclude an equation $nf^a = k$ with a less than 1.90.

Since there will not be room later for further discussion of Plates VI and VII, I must here discharge the obligation of pointing out several curious errors in them and in the accompanying text. At least 13 points are missing on Plate VI; since some of those which are not missing

[9] The Latin index points to more than the actual 8,437 words; the high-frequency divergence shown on Plate IV is of the right sort to explain this. Strangely enough, the same is true of Chinese, as we learn from the full data (Selected Studies, App. C).

are slightly misplaced, it is hard to identify the missing points, but they seem to be those for f ('Peipingese: Number of Occurrences', 257) equals *16, 26–33, 36, 37,* and *50.* Eleven of these fall below the left end of Zipf's line; two lie just above it. Taking the twelve missing points into account, the least-squares line for the first forty points is $nf^{1.55} = k$. This 'best' line has a slope some four degrees different from his line, or swings away from it about 8 mm. (more than a quarter-inch) in the length of his line—since the two lines cross, we add the deviations at both ends. The matter is further complicated by the fact that Zipf gives an erroneous index for the line he drew. His line is $nf^{1.33} = k$, and not $nf^{1.78} = k$ as he states at the top of 258. It is, then, steeper than the line he drew on Plate VII (for which he gives the correct index), and this would have to mean, according to his criteria, that Chinese is more highly inflected than French!

But the data are not such that a comparison of degree of inflection could be based on them. Zipf says (256): 'It is inconsequential for our present purposes that Henmon did not include the relative frequencies of formal prefixes, suffixes, and endings since the high relative frequencies of these would place them above that portion of the curve which is of special interest to us'. There is no warrant for the conclusion he expresses in the words 'since . . . ' , and analogy ought to have led him to believe that the opposite was true—that, just as is true of words, certain formative elements were extremely rare. Who would dare say that *-imes* is 'frequent'? Besides this, Chinese polysyllabism is a sort of synthesis, or aggregation, or 'addition' of morphemes and their meanings, and so is not comparable to the specialization or 'multiplication' (using the word as it is used in symbolic logic) of French inflection.

We are now in a position to appraise Plates I–III. The relation between the frequency of each different word in the Latin, English, and Chinese data on the one hand, and the number of words having the same frequency on the other hand, is correctly (within a measurable margin of error) represented by a straight line on a double-logarithmic chart, by the formula $nf^a = k$ in analytic terms. But the nearness of a to the number *2* is derivative and so-to-say accidental: It is connected with nothing but the straightness of the line and with the fact that the vocabulary is some thousands of words in size, as it would have to be in a sample of any respectable and yet manageable length. In graphical terms, nothing can be significant but the straightness of the line. It might and ought to be asked, even though it did not occur to Zipf to ask the question: Is not perhaps the straightness of the line implied

in the nearness of the index to the number *2*, so that if a curve of that average slope resulted from any count of a sample of different things, then that curve ought to be a straight line? There is a theoretical answer to this (the mathematical reader will already have recognized it in what has gone before), but fortunately we can use instead a practical check. Nearly two years ago the writer (with some assistance) counted the words in two samples of more or less abnormal language, a sample of Basic English and one of Esperanto. The data did not yield straight lines; both lines were curved, and they were curved in opposite directions.[10] As far as we know, then (for this sort of thing can be disproved but never proved), it is possible that straight lines ($nf^a = k$) are uniquely associated with 'natural' language, which Basic English and Esperanto unquestionably are not. But it still remains to be seen whether that is the most appropriate paraphrase of that straightness, and for the present we are left with nothing but the straightness itself.

The lines are straight; the relation between n and f is a power function. Now that may not seem to mean very much, but at least it means just what it says. What the 'philosophical implications' (in more accurate language, the 'possible paraphrases') may be, we are not yet in a position to guess; for the present we are still at the point where we have enough to do in simply arguing about what could be said concerning the fact that the relation of n to f is 'monotone'—that the lines, in going downward, go always to the right—for there seems to be no a priori reason for supposing that there ought to be more hapax legomena than there are of words occurring twice. But if the centuries-long experience of the natural scientist is to count for anything, then the straightness of Zipf's lines will some day prove rich in philosophical implications, and the paraphrases of $nf^a = k$ will be various and frequent in future linguistic works. For straight lines are notoriously among the most valuable discoveries of the scientific observer. Any natural scientist will confirm this. But he will also tell you (if carefully questioned— it is a thing so well known that it is seldom said) that it has not been found profitable to begin the use of each new discovery by using it as the only basis of a new science.

Though there are, as we have seen, plenty of opportunities for inconsistency and neglect of important principles in the mechanics of statistics, the most subtly dangerous errors come in when one begins to paraphrase and argue. As one possible paraphrase of the straightness

[10] This is not surprising: Esperanto shuns metaphor, while Basic English overworks its small vocabulary.

of the lines on Plates I–III, we have $\left(\text{since } n = -\dfrac{dr}{df}\right)$ the 'harmonic structure' of vocabulary (Plate IV and 45 ff.) according to which the mean interval at which a word recurs in connected text increases regularly with its rank, in order of frequency, among all the different words in that text. Zipf properly inquires whether this is connected with the length or phonetic size of the words, and furnishes a set of tables (26 ff.) showing how the sizes of words are, in his experience, correlated with their frequencies. We might have guessed that long words are generally rare (or that rare words are generally long—it can make no difference which way it is said), but it is one of the virtues of statistics—and not the least of its virtues by far!—that it can be used TO MEASURE THE OBVIOUS.   And indeed, whenever the obvious has been measured there is occasion for a goodly amount of philosophising. Zipf is clearly within his rights in setting out to talk about this.

But he forfeits his rights with the first sentence he writes (28), for he starts out by hunting for a causal relation. Of course he finds it, and finds it quickly, for he has already decided[11] what it ought to be.   He thoroughly confuses the synchronic and diachronic aspects of language-description, using paraphrases which might well apply in one or the other aspect but would have to be replaced with more complex ones if both were to be covered together.   And he refers every use of a short word (where a longer one would be possible) to intention—to a conscious or unconscious striving for economy.   Then, of course, the nature and (nota bene) the direction of the causal relation is easily settled: 'This tendency of a decreasing magnitude to result from an increase in relative frequency, may be tentatively named the Law of Abbreviation' (38).   There is nothing hard about this.   Given an observed functional interrelation it is always possible to 'prove' that one variable is the cause and the other the effect—that is the way homo sapiens, accustomed from earliest childhood to having things 'explained', invariably behaves—and with a modicum of the will-to-believe we quickly arrive at a causal Law, with nothing tentative but the adverb.

Of course it helps if we have first decided that one variable must be the cause of the other.[12]   And it helps still more if we summarily decide

[11] Many years ago, that is, as we learn from his Relative Frequency 1, where this illuminating phrase occurs: 'With my *a priori* theory in mind, . . . ' Cf. footnote 21.

[12] As Zipf immediately does (28 f.), forgetting that both together might as well be considered as effects of a third or several other causes, once you start talking about causality. Trained statisticians seldom make that mistake; they are

WHICH is to be the cause of the other, as Zipf does (29) with these words and no more: ' . . . because a speaker selects his words not according to their lengths, but solely according to the meanings of the words and the ideas he wishes to convey. Occasionally, of course, out of respect for the youth, inexperience, or low mentality of a particular auditor, a given speaker may seek to avoid long and unusual words. On the other hand, speakers are sometimes found who seem to prefer the longer and more unusual words, even when shorter more usual words are available. Yet in neither case are the preferences for brevity or length followed without respect for the meanings of the words which are selected'. This is intended to show that shortness cannot be the cause of frequency, which conclusion is taken as a demonstration that frequency is rather to be considered the cause of shortness of words. Now the amusing thing here is that, taking the ideas just quoted and remembering that the same meaning can be expressed in words of different lengths (as Zipf repeatedly says throughout the book), it is equally easy to 'prove' what he dismisses, and to use all his illustrations as illustrations of the (apparent) contrary of what he uses them for. The demonstration can safely be left to any reader who likes to talk about talking.[13] The combination of these apparent contraries is not, however, an antinomy—it is really a tautology.

For as far as we know, the two variables in functional interrelation are ambivalent, in that either may be taken as the cause whenever one feels the urge to find a causal law.[14] Taking each choice in turn and combining the two resultant laws, we have a circular 'explanation' instead of the original linear description. That linear description says just what we know: 'Short words are generally frequent words, long words generally rare', with a footnote to say that this formulation is not

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always on the alert for this their special bête noir, and have given it the pejorative name of 'spurious correlation', under which title the curious will find, in any good book on mathematical statistics, directions for discovering whether there really is any relation between the consumption of imported apples per head and the female cancer death-rate. Zipf states the alternative carefully, but forgets that the Law of the Excluded Middle applies only when the alternatives BY DEFINITION together include all possibilities.

[13] A pursuit which Fritz Mauthner (somewhere in his three-volume Beiträge zu einer Kritik der Sprache) aptly compares to keeping a fire in a wooden stove.

[14] Incidentally, it is precisely that sort of ambivalence which the ideal natural scientist has in mind when he speaks of 'dynamic equilibrium'. He has not demonstrated mutual causality in nature; he has simply perceived a quality of his knowledge.

intended to be in any way different from 'frequent words are generally
short words, rare words generally long'. The 'explanation' says more
than we know, for it introduces, without justification, the notion that
there is a difference between these two formulations—and causality
creeps in with the notion that the choice between the two is not arbi-
trary. The expansion of the linear description into a circular explana-
tion is a tautology, and the Law of Abbreviation, the central principle
of Zipf's doctrine, is an arbitrary half of a tautology.[15]

All tautologies aside, Zipf has presented us with one functional
interrelation, measured another for us, and offered a number of stimulat-
ing paraphrases. As a scientist he may well be proud of having done
that much; the rest belongs to omniscience.

The next section of the book deals with 'the form and behavior of
phonemes'. Since Zipf's phoneme dates from before the appearance of
Twaddell's treatise,[16] it is very hard to follow him sympathetically
through the arguments. Fortunately that is unnecessary, for as far as
they are valid they can be replaced in terms of Twaddell's procedure.
For example, the useful[17] section dealing with 'skewness' (101–6) can be
replaced by the use of micro-phonemic sets;[18] the results will then be in
terms of what is known—the data, the explicit assumptions, and
nothing else. Zipf's arguments depend partly on probably unverifiable
guesses about articulation, partly on an ethical fiction;[19] that is, his
phoneme is an articulatory norm standing in mutual-causality relation
with the speaker's intention.[20]

[15] A great deal more could be said about these things, but fortunately it has
been made unnecessary for me to try to say it myself, and I hasten to give all due
credit, thanks, and appreciation to Leonard Bloomfield, who in all his works and
especially in two recent reviews (LANG. 8.220 and 10.32) has rendered to linguistics
the inestimable service of reminding us of the nature of scientific method.

[16] W. F. Twaddell, On Defining the Phoneme, LANGUAGE MONOGRAPH No. 16
(1935).

[17] Eminently useful in that it states a problem and gives us valuable hints for
the application of a strict procedure.

[18] Twaddell 61 f.

[19] Similar to the fictions called Free Will and Responsibility. (Hans Vai-
hinger, Philosophie des Als Ob, Leipzig 1922[8], 59 f.) Zipf holds the speaker
responsible for having articulated in a certain fashion under the fiction that he
INTENDED to articulate in a certain (perhaps different) fashion. It does not
appear that Zipf recognizes this as a fiction, nor does he recognize what is still
more important, that an ethical fiction cannot be the basis of a scientific method.

[20] Zipf does not DEFINE his phoneme; characteristically, he 'explains' it. The
kernel of his explanation is this: 'The speech-sounds, distributed about the norm

Just as Zipf has 'found' a causal relation between lengths of words and their relative frequencies, so he is prepared to find a causal relation in the same sense[21] between the relative frequencies of phonemes[22] in the stream of speech and their sizes—their 'magnitude of complexity'—so that a rare phoneme would (because of its rareness) be given a complex articulation to make it conspicuous (so that it wouldn't be confused with a well-known common one?), while the commoner ones would be given a less complex articulation.   In preparation for the finding of this causal law, he gives arguments to show that aspirated, fortis, and voiced stops are respectively more complex than unaspirated, lenis, and voiceless stops—he is presently going to show us that the latter are more frequent.   It should be noted that Zipf's 'complexity' is now entirely articulatory (in contrast to his earlier method, where the word was 'conspicuousness'[23]), and is to be determined solely by observation of the act of speaking.   Now in that frame of discourse 'complexity' can only mean 'difficulty', and ought to be measured by the amount of control[24] which the speaker exercises.   On that basis, an unaspirated stop is more complex than an aspirated one, since to avoid aspiration the speaker must begin producing voice or else stop the lung-pressure EXACTLY as the stop is released, though he may do the same SOONER OR LATER after release and still produce a true aspirated stop.   Again, a voiced stop is easier to manage than a voiceless one, since it does not

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of the phoneme, give significance to the phoneme, just as the norm of the phoneme gives significance to the speech-sounds which approximate it.'   (53.)

[21] Zipf found his Law first for syllables and sounds, and presented it as the Principle of Relative Frequency on page 4 of his Relative Frequency.   In the present book he derives it first for words, which is easier to do.   Apparently he got the idea of the universality (or versatility) of the Law in the interesting fashion stated in the quotation referred to in my footnote 6.

[22] Space is lacking here for explicit treatment of many theoretical questions— e.g., whether a phoneme 'occurs'—but I shall try to cover them by implication. So much can be said, however: Zipf's difficulties show the necessity of a rigid procedure for determining phoneme-membership.

[23] Relative Frequency 36 f.   It was a phonetic—largely acoustic—conspicuousness, and was to be measured principally by the amount of attention it attracted. Here the frame of discourse is SOCIAL.   Now in social behavior, practically by definition, conspicuousness is the same thing as rarity—for it is only unusual behavior that attracts special attention.   The proper definition of 'conspicuousness' could hardly be anything but 'that which varies inversely with frequency'.

[24] The amount of EFFORT is irrelevant, unless we are prepared to find that more complex phonemes are more frequent in accented syllables than in unaccented ones.

require cessation of voice after a preceding vowel, etc.—a conclusion which would find general favor among Romance scholars.

The other preparation for the introduction of the data is a demonstration of the propriety of associating English [t] with French [t], English [d] with French [d], for statistical purposes, in spite of differences in articulation. The association is done according to resemblances (in that [t]^e is closer to [t]^f than to [g]^f etc.); since the appearance of Twaddell's monograph we must dismiss this, and for quite different reasons than those which Zipf cites as possible objections. I see no reason, however, why Twaddell's method should not be extended to this field, if any good purpose can be served thereby. Then [t]^e could be statistically associated with [t]^f according to the proportion [t]^e : [d]^e :: [t]^f : [d]^f as soon as it has been shown that in each language there is a two-member series of tongue-tip stops. The same cannot be said of an attempt to associate Burmese [t] with Cantonese [t], for the Burmese phoneme is in a three-member series while the Cantonese series has only two members. If this needs support in the minds of those who do not accept Twaddell's procedure, let it be the consideration that very likely Burmese [t] would have special articulatory peculiarities, to keep it apart from BOTH [t^h] and [t$_1$], which would be unnecessary in Cantonese.[25]

The data which Zipf presents (68–79) are intended to show statistical correlation between two variables OF WHICH ONE IS A CLASSIFICATION (for Zipf admits the impossibility of measuring complexity and does no more than classify into 'more' and 'less' complex). Now that sort of correlation is recognized in statistics, and there are well-grounded methods for attacking the problem. But they are founded on a condition which must never be forgotten: Before using the classes, one must really classify. There must be a classifying procedure founded on data, assumptions, and water-tight logic. What is wanted here is a uniform classifying procedure which, applied to the three articulatory oppositions mentioned, would infallibly determine which member of each opposition is to be called 'more complex' (or called by any other factitious name). When such a procedure is lacking—and I think I have shown that it is lacking here—THE CLASSIFICATION ITSELF must be based on statistical study. It can be done, and has been done repeatedly.[26]

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[25] The connection between the two points of view is implied in Twaddell 57.

[26] A classical example is Karl Pearson's article On the probability that two independent distributions of frequency are really samples of the same population, Biometrica 10.85–143 (1914–15).

The result, as the statistician knows, is a circularity which must then be carefully collapsed into a linear description—the reverse of Zipf's favorite procedure. Zipf here chooses the harder way (perhaps the impossible way) and then makes it easier for himself by the standard magician's device of putting the egg in the hat before the audience knows he is going to take anything out of the hat.

In view of all this, the particular uncertainties in the data are beside the point here, and they have already been partly treated by the reviewers of Zipf's previous books. At least one definite improvement ought to be noted: the Chinese stops now have a more acceptable treatment than before. But this is set off by the fact that the Czech data are now cited as being 'from ACCURATE PHONEMIC transcriptions' (74), though the same data are given in his first publication (to which he here refers us for the sources) as representing 'printed letters'.[27] The footnote in the present book contradicts and partly corrects the misstatement with the words ' . . . the conventional Czechish alphabet is practically as accurate a phonemic alphabet as can be devised for Czechish'. The correction is not complete, for there is good warrant for considering Czech (as also Russian and Bulgarian) b d (g), when final or before voiceless sounds, as belonging to the [p] [t] ([k], [x]) phonemes. When we note that Zipf went to a great deal of trouble[28] to take account of the similar peculiarity in German, we can only conclude that he was not sufficiently on his guard against possibilities of ignorance or inconsistency; we may even be pardoned for suspecting that he simply hadn't been told about the Spanish orthographic equality $v = b$.

There are three more principal divisions of the present book, dealing with 'accent', 'the sentence', and 'the stream of speech and its relation to the totality of behavior'. Since they are based on what has already been discussed, and since the Law of Abbreviation determines the argument throughout, further detailed criticism is hardly necessary. As elsewhere, there is a great deal of stimulating discussion and suggestive formulation. But its claim to permanent scientific value is vitiated by the practice of working out an unambiguous causal explanation from and for each ambivalent correlation. Such 'explanations' may be as

[27] Zipf, Relative Frequency 42 (top) and again (45) at the head of the table. The Czech source was Těsnopisné Rozhledy ('Stenographic Survey' or 'Review'), so that we should expect the data to represent letters and not phonemes, even though Sedláček was the author.

[28] Relative Frequency 52–6.

coherent and logical as you please; they are not scientific demonstrations. As Bloomfield so neatly puts it, they 'short-circuit inquiry'.

In his Introduction (5) Zipf has written: ' . . . it is difficult to believe that linguistics has been entirely mistaken in the direction which it gave to language study. Certainly no student of speech-dynamics can for a moment regret the stringency of the historical and comparative disciplines which have provided him with immediately available material'. This bears an implication, unjust both to statistical method and to linguistic science, which must be explicitly denied. Statistics enjoys no qualitative superiority over the best practice of modern linguistics. It is simply a technique for so describing data that their regularities are given consistent labels. The statistician chooses to work mathematically because mathematics, which is definable as 'pure consistency', furnishes a conveniently available array of consistency-patterns. His technique has nothing to do with causality—not even with that mystic mutual causality for which the label 'dynamic equilibrium' has been borrowed from science. Those are limitations of all scientific method. Anything beyond this is not science, however worthy it may be as artistic description—e.g., as 'explanation',—and when those limits are overstepped, science has a right to disclaim the result and refuse any possible blame.

Although we cannot ascribe to the statistical method any sovereign efficacy in linguistics, it by no means follows that a sound use of statistical method is out of place in our discipline. One of the objects of this critique has been the habilitation—under the circumstances one might even say the re-habilitation—of a branch of scientific method, the statistical, as a tool in linguistic study.

MARTIN JOOS

De Hettitische *ḫ*. Pp. 43. By WALTER COUVREUR. (Teksten en Verhandelingen, Nummer 12; Beheer van Philologische Studien.) Leuven, 1935.

Coming from a study of de Saussure's[1] mathematical speculations about the origin of the Indo-European long vowels, Couvreur, like several other scholars, found unexpected confirmation of the theory in the Hittite sound (or sounds) written with the cuneiform *ḫ*-signs. He tells us that his theory was completed before he became acquainted with the views of Kurylowicz, Cuny, and Pedersen, and he differs from their conclusions in several respects.

[1] Ferdinand de Saussure, Mémoire sur le système primitif des voyelles dans les langues indo-européennes 134–84 (1879).

Couvreur assumes three laryngeal consonants for primitive Indo-European, ' (= glottal stop), h (= laryngeal voiceless spirant), and ' (= laryngeal voiced spirant). As far as I can judge, his conception of the two last-named sounds is approximately the same as Sapir's (see LANG. 10.276 fn. 4; 12.186), and, to avoid confusion, I shall employ Sapir's signs, namely x and γ respectively. The theory is, briefly, that ' had no effect upon the quality of a neighboring vowel, but x changed an original e to a, and γ changed an original e to o. All three consonants were lost in the Indo-European languages, with compensatory lengthening of a preceding vowel in case a consonant had followed the laryngeal. In Hittite, however, x appears as h^2 (written hh between vowels), and γ as h^2 (written single even between vowels).

A sharp difference from the latest form of Kurylowicz's system[3] is that Couvreur, following de Saussure, derives IE ϑ from ', x, and γ.[4] In other words, the original groups e', ex, and $e\gamma$ had zero-grade forms entirely parallel to those of ei, eu, etc. $(ei : i = e' : ' = ex : x = e\gamma : \gamma)$. Quite consistently he finds ϑ beside $\bar{e}$ ($<e'$) appearing in Hittite as a (e.g. $asanzi$ 'they sit'), but where Indo-European shows ϑ beside $\bar{a}$ ($< ex$) he finds in Hittite h (e.g. $sanh$- 'strive' : Skt. $sanit\bar{a}$ 'winner'). On the other hand he assumes complete loss of ' between consonants in Skt. $dadhmas$ 'we place' and in Hitt. zi-ik-ki-iz-zi (t-ske-zi) 'he is placing' beside dai 'he places'. This occasional loss of ϑ is, of course, commonly assumed by Indo-Europeanists, and Kurylowicz now holds that all laryngeals were regularly lost between consonants, while ϑ resulted from a reduced vowel after the laryngeal (before it, I should say—see LANG. 12.143). In my opinion the h of $sanhzi$ 'he seeks' is due to the analogy of the 3 pl. $sanhanzi$. The normal Hittite correspondent of European a = Skt. i is to be seen in $pahhsnu$- (2 pl. pa-ah-ha-as-nu-ut-te-ni, pret. 3 pl. pa-ah-sa-nu-ir) 'cause to endure' beside $pahhs$- 'protect, keep, be true to' : Lat. $p\bar{a}sco$, $p\bar{a}stor$; for the suffix nu demands reduced grade of the root.

Kurylowicz (op. cit. 254 f.; cf. LANG. 12.141 f.) is also ahead of Couvreur in recognizing the necessity for a fourth laryngeal, which

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[2] I shall omit the diacritic, except in precise transcription of cuneiform signs.

[3] Études indo-européennes 1.27–76, 253–5. Cf. my review in LANG. 12.141–4.

[4] Both Kurylowicz and Couvreur assume that the loss or alteration of the laryngeals occurred in the Indo-European period. Since Hittite is assumed to have retained at least one of the laryngeals, this would seem to necessitate the acceptance of my Indo-Hittite hypothesis, but neither one of them explicitly chooses that horn of the dilemma. Sapir holds that the laryngeals persisted for varying periods in the separate history of the various languages, an opinion which obviously comports with rejection of the Indo-Hittite hypothesis.

changes *e* to *a* but which is lost in Hittite.   For this Sapir writes ˀ; it
was apparently a glottal stop with velar color.   After giving his table,
for the development of initial laryngeals in Indo-European and Hittite
Couvreur remarks (15): 'To these rules there are no exceptions.   Sturte-
vant's etymologies that conflict with our phonetic laws (e.g. *ak-* 'die' =
IE *aĝ-*, ἄγω, *ago*) are wrong.'   I no longer believe in the etymology here
cited; it is not included in my Hittite grammar.   But what of Hitt.
appa 'back, afterwards, again' : Gk. ἀπό; *alis* 'white', *alpas* 'cloud' : Lat.
albus, Gk. ἀλφός 'dull white leprosy', Lith. *alvas* 'tin'; *arkanzi* 'they
shut in, ward off' : Gk. ἀρκέω 'ward off', Lat. *arceo* 'keep away, shut
in'; *arwa(e)-* 'bow down' : Gk. ἀράομαι 'pray, curse'?   For Hitt. *a*
(without *h*) beside IE *ā* before a consonant one may cite *stappi* (*iš-
tap-pi, iš-da-a-pi*) 'shuts in, stores away' : Skt. *sthāpayati* 'causes to
stand'.[5]

Couvreur concludes (14) that original initial γ*e* gives Hitt. *ha-*, IE *o-*
(*haras, haranas* 'eagle' : Gk. ὄρνις 'bird'; *hark-* 'be destroyed' : Ir.
org- 'destroy, kill'; *hastai* 'bone' : Gk. ὀστέον, Lat. *os*), and he very
naturally searches then for intervocalic *h* from γ, expecting that this *h*
would be written singly, just as is *t* (*d*) for original *d* or *dh*.   He finds
but a single example, namely *lahw-, lah(h)u, lah(h)uw(wa)-* 'pour' :
IE *lou-* 'pour'.   It is true, as Couvreur says, that these words are
usually written with a single *h*, but the double writing is not rare in the
verb.   Furthermore the noun *lahhus* 'basin' shows *hh*, and so does the
probably related *lahhurnuzzi*, name of a kind of sacrificial table.   There
are two objections to tracing *h* in this root to γ.   (1) It seems to me
certain that Hitt. *da-, ta-* 'take' is to be identified with IE *dō-* (< *deγ-*)
'give'.   The Hittite conjugation of *da-, ta-* parallels that of *dai-, te-*
'place' much as IE *dō-* parallels *dhē-* in most of the historic languages.   I
discussed the connection of the meanings 'give' and 'take' in LANG.
3.216 f., but it is enough to point to the parallel presented, by Gk. νέμω
'distribute' : Goth. *niman* 'take'.   There is, however, no trace of *h* as a
part of the root of Hitt. *da-, ta-*.   Apparently, then, γ was lost in Hittite
in medial position.

(2) I have tried to show (LANG. 12.181–7) that intervocalic single *h*
represents original ˀ in *sehur* 'urination, urine' : Lat. *sēmen, mehur*
'time, point of time' : IE *mē-* (< *meˀ-*) 'measure'.   I suspect that *h* in
lahw- 'pour' comes from the same source.   In other words, I believe

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[5] Most of the Hittite forms from **steˀ-* (> *stā-*) may contain an analogue of
IE ǝ; e.g. *tittanu-* 'place', which Couvreur (18) ascribes to the root **dhe-*(< *dhe'-*),
although *t* from *dh* is never written double.

that the *o* of Gk. λόω is due to ablaut, and that the base to be assumed is *le'eu-*. If so we may assume *lo'w-* as the basis both of Gk. λόω < *λόϝω and of Hittite *lah(h)w-*. It is disturbing to find Hittite *h* from ' sometimes written double in this word, while it is always written single in *sehur* and *mehur*. I suspect that the peculiarity is somehow connected with the fact that *h* after *a* is usually the representative of original *x* and is in that case regularly written double.

Couvreur's monograph is another indication of the strong drift toward de Saussure's theory of the long vowels, which has been induced by the study of Hittite *h*. The author is the first[6] to advocate in print a double value for Hittite *h*, and there are many details in which he has improved our understanding.

E. H. STURTEVANT

The Prae-Italic Dialects of Italy. Three volumes. By R. S. CONWAY, J. WHATMOUGH, S. E. JOHNSON. Cambridge: Harvard University Press, 1933.

Linguistics is deeply indebted to the authors, to Harvard University, and to the British Academy for these splendid volumes. Of the IE dialects once spoken in Italy that do not belong to the Italic branch of the family the records are published in widely scattered places, and are not always accurately described. The primary purpose of the present work is to overcome these difficulties, and thus prepare for the linguistic interpretation of the material. To say that this plan has been carried out successfully is an immediate indication of the extent of our indebtedness.

The most important of the records are the inscriptions; almost all of these have been seen by one of the editors, and the results of their observations are reported with the most meticulous care. The value of the reports is enhanced by the fact that earlier readings are, when necessary, explicitly criticized. Facsimiles are not employed to attest the readings, because, as Conway well argues (1. viii): 'A facsimile . . . is worth just as much as the skill, knowledge, and good faith of the editor who presents it, and no more; and his knowledge may often be expressed more safely in an articulate form.' Corrections of earlier readings are offered continually, so much so that it is needless to cite examples. In addition there is a detailed presentation of the historical and archaeological background of these documents. The other class of

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[6] Aside from Sapir's brief remark in LANG. 10.276 fn. 4.

material—glosses and proper names—is presented in exhaustive collections.

The first volume opens (1–201) with Conway's edition of the Venetic inscriptions which includes (185–201) an outline of Venetic grammar. His earlier suggestion that the interpuncts, as in *zona.s.to*, are accent marks is repeated in the belief that there is 'no room for doubt' except about details.

At present so little of Venetic is understandable that proof and disproof are impossible. There is, however, the inherent improbability that Venetic should have recorded a feature of pronunciation, which received no recognition in any of the related systems of writing until Greek scholars—some centuries later—became interested in the subject. The supposition that their work—and that of the Hindu grammarians— was under Venetic influence does not strengthen the case. What resemblance there is between the three systems is purely negative: none of the three mark the accented vowel itself; what each actually does is different. The same difficulty is met by the interpretation of the points as indicative of syllabic division, for this compare Vetter, Glotta 15.11 (1927); but it seems much more probable, and the care with which Conway reports the puncts furnishes at all events an admirable basis for a renewed examination of the question.[a]

In dealing with an inscription (no. 159) from Monte Pore, *n'* and *ic* are employed to represent signs which may, as Pauli believed, be merely variant forms of *n* and *k*. If the transliteration is overcautious, it at least errs on the safe side; but the principle is not heeded in a case of greater importance. One symbol *h* represents three Venetic letters: (1) a letter clearly derived from the *h* of the Etruscan alphabet; (2) a single hasta between two puncts; (3) three hastae. It is probable that (2) and (3) are merely graphic variants, but (1) is clearly different; on the other hand when the puncts of (2) are circular rather than linear, or are of varying size, the transliteration *.i.* is employed,[1] tho this variation too may be merely graphic. The underlying difficulty seems to be a failure to distinguish sharply between transliteration and transcription.[2] As a matter of transliteration it is desirable that a

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[a] The above was written before access to Vetter's article, Glotta 24.114–133 (1935–6) which seems to give the final solution. The use of the puncts is due to a shift from syllabic to alphabetic writing; they thus come to indicate the division of syllables. Conway's statements about the puncts seem to need more correction than I had expected in view of the great attention bestowed upon them.

[1] Cf. nos. 142, 149, 161.

[2] Cf. Bloomfield, Language 89–90.

distinction be drawn between these characters and I shall here employ
h for (1), *h* for (2), and *h*¹ for its (possibly) graphic variants.

The transcription of these symbols is one of the most important
problems of Venetic. Sommer, IF 42.90–132 (1924), suggested that *h*
be transliterated *.i.,* which is what it would seem to be graphically—
an *i* between two puncts. For it he recognized three values, which
would need to be distinguished in transcription: (1) an [i] sound, plus
of course anything the puncts may denote; (2) in the digraph *vh* that is
used for an [f] sound; and (3) to indicate the length of a vowel. Vetter
endorsed this enthusiastically, Glotta 15.10 (1927); but later recanted,
ib. 20. 39 (1931). Unfortunately Conway's conviction that the points
are accent marks caused him to feel (25₁) that a discussion of Sommer's
suggestion is needless. Whatmough (2.268) seems to be of the opinion
that the question is still open, tho he refrains (2.511) from discussing it;
I am inclined to regard Sommer's suggestion as the most promising
solution.

The varying uses of the character seem explicable. Indo-European
[-kt-] was treated in Venetic as secondary [-kt-] is treated in Umbrian;³
that is it became [-it-], presumably after passing thru [-ht-]. When the
Veneti learned writing from the Etruscans, they had no [h] sounds; but
they could use the letter **h** in the digraph *v***h** [f], and also, as the Um-
brians did, to indicate the length of vowels. Later, when [-kt-] had
become [-ht-], they had a new use for the symbol. Still later, when the
stage [-it-] had been reached, there would result a conflict between a
historical spelling -**h***t*- and a phonetic spelling -*ht*- with the result that
h and *h* would seem to be interchangeable.⁴ The usage would then be
simplified by eliminating one or the other letter; and there seem to be
local differences in the way this worked out. At Este and most other
places **h** disappeared. I have noted only the following survivals of **h**:
at Este on two (nos. 119, 120) tomb inscriptions (the vases in them use *h*),
and in no. 111 which Conway considers votive, while the Museum
treats it as sepulchral; at Padua in no. 148 'probably a tombstone',
and in no. 150 *vh*aχ·*s*·θ*o*; not to mention the doubtful reading in no.
145. All these examples concern the digraph *v***h** and are (or may be)
sepulchral except for the potter's signature (no. 150); exactly the sort of
inscriptions in which old fashioned writing might be expected to persist.
The scanty material from the neighborhood of Trieste gives a different

³ Cf. Buck, Grammar of Oscan and Umbrian 89. The omission of this book
from Whatmough's list of reference works (2.xxv) is curious.

⁴ Two circumstances made this all the easier: *h* is again merely a diacritic;
the oscillation would occur in one of the most frequently written words—*Rehtia.*

picture: no. 164 uses only **h**, *lah·v·na***h** *vrot·a·***h**; no. 165 only *h¹*, χ*ah¹vo·s·*.
In the Gurina *h¹* is found nos. 166, 176; *h* in no. 167.[5]

It was the inscription *lah·v·na***h** *vrot·a·***h** that caused Vetter, cf. Glotta
20. 73 (1931), to abandon Sommer's idea. In these forms he recognizes
datives such as those seen in *rehtiiah, śahnateh*; and since **h** goes back to a
symbol originally representative of [h], he interprets these forms as
[-a:], and being unwilling to separate *rehtiiah* from them, denies that
it could have ended in [-ai]. Against this can be set the dative form
voltiχenei in no. 126, an inscription which uses no puncts, as compared
with]χ*eneh* in no. 125, which does use them. These seem to prove an
ending [-ei], to which [-ai] would be parallel. Both these inscriptions
come from Este, and I think that the *-ah* of the Trieste neighborhood
shows no more than a local divergence of graphic habits.[6]

The difficulty I find may be stated as follows: if *h* represents some [i]
sound we may expect to find words ending in consonant +*h*, beginning
with *h*+ consonant, containing *h* between consonants; but no such exam-
ples occur. This is, however, offset by the fact that examples of [i] other-
wise written are not frequent in these places. I have noted: (1) *enoni*
in no. 157; *vo·χ·sii* (no. 25) which Solmsen took as a genitive, while
the glossary suggests an abbreviation for **vo·χ·siiah*; more uncertain are
slχkii in no. 36 'a mock inscription', and *stati* no. 154, not to mention
vii in no. 185; (2) only *iθuria* no. 127; (3) *kolivhiila* (no. 15) or *koliahiila*,
-kiitv no. 36 'mock inscription', *visehioφoh*[(?) and *voltiχnos* no. 123,
voltiχenei 126, and more frequent examples after Este is left: *śminθiakśke*,
kravil mentioned on page 124, *ekupeθaris* nos. 141, 142, cf. 157, *olloukiθ*
no. 143, *vennonis* no. 144, *θihavos* no.148, *vhaestinioh* no. 149, *pilpoθeh*
kuprikonioh no. 149a, *śminθ* no. 151a, note iv, *mannisnauius, udisna*
no. 153a, *siirka* no. 156, *n'ostin'eh, niskarikor* no. 159, *kreviniai osites*
no. 161, *tisri* no. 177, *osiśar* no. 183, *φiχos* (?) no. 183. *kovezikos*(?),
rośicos no. 186. Nor are *i*-diphthongs at all frequent: *oi.p.* in no. 18, if
correctly read; *einaim* in no. 111, a 'very doubtful' reading; *neirkah*
no. 111, otherwise read by others, and probably is, or should be, *nerkah*,
cf. nos. 26, 32, 34; *ve·i·χnoh* no. 142, *ve·i·ne·s·* no. 161, contain what
may probably be simply a graphic variant of *veh-*; *von'aico* no. 159,
perhaps merely *von'ako*. At the end of a word I have noted only:
kreviniai no. 161, if this is the correct division; *koi* no. 162, which may

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[5] In no. 156 from Treviso iiiv is probably not *h¹u*; Conway suggests numerals
as an alternative interpretation.

[6] Why is a dialectic differentiation of the sentence doublets [-a:], [-ai] impos-
sible; one being generalized in one locality, one in another?

also be read *kol* or *kov*; and *ontei·appioi·sselboisselboi* in no. 157. The last inscription is known only from a copy made in 1781, and very probably *onteh* etc. was its reading. Since [i] is so infrequent, not much weight can be given to the fact that examples of [i] with puncts do not occur under the conditions named at the beginning of this paragraph.

If the above is a true picture, Venetic vocalism is quite different from that of Indo-European. There will be need of correction in Conway's statement (197) that bar *lehvo·s·* cognate with or borrowed from λαιϝόs cf. *Laeuus,* 'there is so far no evidence of any departure from the original vowel qualities of Indo-European in either accented or unaccented syllables'. The need will be less if *h* symbolizes at times an [i] sound, and I would agree with Sommer that this is the case.

The second part of the volume contains (209–439) a collection of the ancient names (local, divine, and personal) of north Italy by Mrs. S. E. Johnson (née Jackson), to which Whatmough has added (440–59) a similar listing of the names of the Raeti.

The second volume is almost entirely the work of Whatmough. It opens with a presentation of the records. First come those of the Raeti (3–64), inscriptions with the exception of a single gloss.

Raetic is for Whatmough (4,580₁) not Etruscan, but a West Indo-European dialect that would be connected by isoglosses with Celtic, Illyrian, and Germanic, and that has borrowed from Etruscan. His phraseology is not happy; and would, on the surface at least, suggest a confusion of linguistic, cultural, and biologic grouping. Compare on the position of Raetic, Thurneysen, Glotta 21. 1–7 (1932); Whatmough, ibid. 22. 27–31 (1933). Devoto, Idg. Jahrb. 19. 182 (1935), still classes Raetic as non-Indo-European; and it may be noted that Walde-Pokorny has found nothing usable in Raetic. Stolz-Leumann 43 is also non-committal, and it seems best to regard the question as still open. If *ist.i.* (189 bis) could be equated with ἐστί, the problem would be solved; but, as nothing else in the inscription can be interpreted, this suggestion, tho 'attractive' to Whatmough, must be regarded merely as a guess.

The next section (65–165) headed Kelto-Liguric brings the Lepontic inscriptions and the Ligurian glosses. For Whatmough Ligurian is Indo-European, and that is again debatable ground, since Hirt, Idg. Gram. 1.43, most positively maintains the opposite. It is probably a question of more or less, with the material insufficient to permit accurate gauging. One of the clearest bits of evidence for the presence of Indo-European-speakers among the Ligurians, the story about Ἄμβρωνεs,

is classed by Whatmough as doubtful, but has been utilized by Kret-
schmer in a brilliant article, Die Herkunft der Umbrer, Glotta 21.112–24
(1932).   On the other hand the 'well attested' σιγυννα- is not Ligurian,
cf. Vetter, Glotta 20. 43 (1931).   From Ligurian as known by glosses
and proper names Whatmough properly distinguishes the language of
the 'Lepontic' inscriptions.   This is unmistakeably Indo-European,
but not to be classified with any other branch of the family; that it is,
as Whatmough believes, a celticized form of Ligurian is probable
enough.

The next section (166–206), headed 'Gallic', is of doubtful value.
Except for the republication of three inscriptions, it consists of a collec-
tion of glosses.   The editor's hope is to distinguish the speech of Cisal-
pine Gaul from the Gallic spoken elsewhere, on the principle that
'words of Gallic origin found in authors older than the conquest of
Narbonese Gaul' (118 B.C.) must belong to the Cisalpine dialect.
However, since the collection is limited to the words for which we
happen to have a statement of Gallic origin from some Roman, it must
be necessarily inadequate for the purpose.   Even use of the discoveries
of 'modern etymologists'—here intentionally disregarded—would leave
us with an incomplete picture because of the fragmentary nature of our
information.[7]   Gallic might well in this work have been treated like
Etruscan—ignored completely.

The following section (207–57) deals with a few inscriptions from
East Italy, the southernmost of which have previously been called 'Old
Sabellic' among other names.   The northern group is written in a
different alphabet; and, in Whatmough's opinion, are in a different

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[7] A few matters of detail: Latin *albus* 'white' is not borrowed (180), but a
continuant of primitive Italic *alfos.   The same cannot be true of Sabine *alpus*,
supposing that Festus has reported it correctly.   It is still an enigma.   Possibly
the Sabines borrowed ἀλφός as a medical term 'white eruption', and thus gave
Festus what seemed to him a chance to explain *Alpes* 'a candore nivium'.   *Alpes*
is probably non-IE, cf. Walde-Hofmann 33; Walde-Pokorny 1.93

Latin *offendimentum* (186) is a ghost-word, the careless copying of *offendit
mentum*, cf. Stolz-Leumann 242.

Latin *caterva* 'legio' (187) is not borrowed, cf. Walde-Hofmann 181–2.

Latin *cercius* 'the mistral' (188); the form cited is from Cato, and probably
hyper-urban for *circius* otherwise attested.   This is borrowed not from Celtic,
but from κίρκιος; cf. Walde-Hofmann 220.

Latin *sparus* 'spear' (193) need not be considered a borrowing, cf. Walde-
Pokorny 2.665; nor do the passages cited state that the word is Gallic.

Under *Carissa* 'vafer' (195) read: 'from *Car*, like *Cilicissa* from *Cilix*'.   If the
word is Celtic it probably came through Etruscan, cf. *favissa, mantissa*.

language.   None of the inscriptions can be read, and the guess that they are Illyrian is regarded as the most probable.

With the records of Messapic (258–430) we come to a subject that Whatmough has made peculiarly his own.   With Whatmough's work before him, Ribezzo has published a fascicle of his Corpus Inscriptionum Messapicarum (RIGI 19.57–80 [1935] and thus afforded an opportunity of testing the accuracy with which Whatmough has reported the readings of the records—which from the self-imposed limitations of his work is after all the most important question.   The test is, as far as it goes, quite reassuring.   Whatmough's reports of what he has seen are never questioned; differences arise only where he is dependant on Droop, or where there is question of choosing between conflicting reports given by early scholars of inscriptions now lost.

Exceedingly interesting is the section (431–500) on Sicel.   The inscriptions do not yield much, but the glosses seem to show that Sicel was either an Italic dialect, or at least the continuant of a language which had borrowed from primitive Italic.   The clearest evidence is λίτρα from prim. It. *līprā* (> Lat. *lībra*), itself probably borrowed from some non-Indo-European language.[8]   Next to be mentioned is μοῖτος: prim. It. *moitos*, a word that has disappeared from all Italic dialects, leaving only a derivative, Latin *mūtāre*, as evidence of its former existence.

'Ρογός 'barn, granary' cannot, as Walde-Pokorny 2.263 points out, be cognate with Latin *rogus* because of the lack of a prothetic vowel. It must be borrowed from Italic; and, on account of the divergence of meaning, not directly from Latin.   The word is cited from Epicharmos; and, tho it is found also in Heraclea (Bechtel, Gr. Dial. 2.287), may well have come into Greek from the Sicels, the connection with Latin being in either case only through primitive Italic.   In other cases slighter deviations of meaning do not exclude the possibility that Sicel has borrowed from Latin: κάμπος probably means 'race-track', not one of the light-armed troops mentioned by Gelo (Hdt. 7. 158),[9] and is

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[8] The fractions such as τετρᾶντα can be explained without difficulty from Greek: < *τετρα-ϝεντ-, cf. Brugmann, Gr. 2.1.464, where parallels from Avestan are cited.   There is no occasion to look upon them as 'hybrid forms'—Greek stem and Latin (cf. *quadrans*) suffix.

[9] To believe differently is to put too much trust in the accentuation of Hesychius' manuscripts.   'Ιπποδρόμος occurs only in this passage of Hdt. and would itself need glossing.

probably borrowed from Latin *campus*; κάτῖνος 'platter': *catīnus* 'deep dish' is a similar case.[10]

The difficulty of gauging the relation of Sicel to primitive Italic is increased by the facts: (1) many words of Sicilian Greek are Greek with a limited range of attestation; and (2) there have been borrowings in many directions. Thus ἄζετον underlies Phocian ἀζετόω 'to find guilty' and seems explicable as < *ἀ-δἰ-ετος 'not to be released, guilty'.[11] 'Ασχέδωρος 'boar': Kretschmer's etymology must be improved to *ἀν-σχε-δωρϝος, as compensatory lengthening would not take place after the loss of ϝ in the dialects in question, cf. Bechtel, Gr. Dial. 2.221. After that there is no more to say. 'Αμορϝίτης is a doubtful attempt to reconcile ἀμορβίτης (Ath.) and ἀμοργίτης (Hesych.). Even in Epicharmus' time no trace of ϝ in such combination was left, cf. Bechtel, Gr. Dial. 2.220–1. The suffix -ίτης was very productive of names for foods, cf. Debrunner, Gr. Wortbild. 180: and the chances are that ἀμορίτης (: ἄμορα), ἀμορβίτης (: ἀμορβός), and ἀμοργίτης (several possibilities for the underlying word) are related only in their suffix. Similar in its sphere is the suffix -ίας, cf. Debrunner 145, with which ὀξερίας 'cheese' is formed. Whatmough says (463): 'If derived from the stem of ὄξος (-εσ-), this form, though Greek, would appear to show -ρ- from intervocalic -σ-'. The suggestion is impossible, and emendation seems necessary: ὁ ξερίας has already been proposed, while ὀξηρίας and ὁ ξηρίας (cf. ξηρὸν γάλα 'cheese') are other possibilities. Sicilian Greek λέποριν, Latin *leporem*, Massaliot Greek λεβηρίδα all point to a non-Indo-European *leper*- taken from some language that probably had only one phoneme for [p] and [b]. Consequently they have nothing to do with rhotacism. Latin *lepus* is an analogical formation, the reasons for which are not clear. Nor is the route of the borrowing clear. The possible complications of borrowing in this region may be illustrated by Sicilian Greek νοῦμμος borrowed from Sicel, which borrowed from Latin *nummus*, borrowed in turn from νομιμός. Or by ἀβολείς, "Αβολλα borrowed from some Sicel forms which may either be cognate with Latin *abolla*, or borrowed from it; since in this case the Latin word cannot (cf. Walde-

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[10] Whatmough's 'receptacle for money' seems to rest on a mistranslation of Varro, LL 5.120: ubi pultum aut iurulenti quid ponebant a capiendo catinum nominarunt, nisi quod Siculi ubi assa ponebant.

[11] Less probably from ἀν-ζετω-, Brugmann-Thumb 87. On consonantizing of ι cf. Kretschmer, Antidoron 193. That there is trouble with the definition ἄπιστον is recognized by Bechtel, Gr. Dial. 2.152. I should regard it as a rough translation colored by the context.

Hofmann) be borrowed from ἀναβολή. An example of a borrowing in a different direction: γέρρον 'wicker-work' and in a narrowed meaning 'shield' developed in Sicily a marginal meaning 'γυναικεῖον αἰδοῖον'. The change may seem surprising, but as a matter of principle[12] we have learned that we must not set ourselves up as judges in such matters. The changed meaning is well-attested, and since blades pierce shields as well as go into scabards it is in reality no more surprising than the meaning of *vagina*, cf. Plautus, Pseud. 1181: *conveniebatne in vaginam tuam machaera militis*. The use of taboo-words as derisive or contemptuous exclamations is familiar[13], and γέρρα as such was borrowed in Latin *gerrae*, 'Nonsense!' That Latin also borrowed *gerrae* 'crates vimineae' from standard Greek is possible, but not well attested. The only example cited is Paulus ex Festo 83.1 where it introduces an aitiological story to connect *gerrae* 'Nonsense!' with γέρρα 'wicker-work'.

The remainder of the book contains: a discussion (501–43) of the origins and interrelations of the alphabets used in these inscriptions; a commentary (544–79) kept brief, for on principle the interpretation of the documents is reserved for a later occasion; notes (580–610) on orthography, phonology, and word-forms of Raetic, Lepontic, and the Gallic inscriptions of Italy, and with greater fullness, on the grammar of the Messapic inscriptions; an appendix (611–26) on alien, spurious or doubtful inscriptions; and addenda (627–32).

The third volume is given over to the Indexes. Of these special mention must be made of the Glossary to the Dialects (3–56) compiled by Whatmough except for Venetic items by Conway. It is a very laborious and careful piece of work, whether one agrees with the suggestions contained in it or not. I have used it considerably and have yet to note an omission or an error.

The work is a splendid achievement; a monument to Conway, and a cause for congratulation to Mrs. Johnson and to Whatmough.

G. M. BOLLING

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[12] Cf. Bloomfield, Language 150, 435.

[13] Very modern American *nerts* is a refinement of such, and the same may be true of the older *Oh shoot!*

NOTES AND PERSONALIA

THE COMMITTEE ON NOMINATION OF OFFICERS FOR 1937 consists of Prof. D. B. Shumway, University of Pennsylvania, Chairman; Prof. E. Adelaide Hahn, Hunter College; Dr. Truman Michelson, Smithsonian Institution. According to the plan outlined in LANGUAGE 6.267–8, members of the Society may send to the Chairman of the Committee their suggestions for nominations for the various offices; these should reach him not later than October 5, in order to receive due consideration.

THE LETTERS which come from foreign scholars on our complimentary list are always appreciated; and we take pleasure in transcribing here a portion of one recently received:

Bonn, 14.4.36: Vor etwa vierzehn Tagen erhielt ich die beiden letzt erschienenen Hefte der Language-Veroeffentlichungen (Volume 12.1 und Supplement). Ich moechte diesen Anlass benutzen um Ihnen nicht nur für diese Hefte zu danken, sondern auch für die vorhergehenden Veroeffentlichungen der Linguistic Society of America. Es ist eine ungemein reichhaltige Fülle an Material und Bearbeitungen, die ich dauernd zur Hand habe und aus der ich immer frische Anregungen schoepfe. Stets begrüsse ich mit neuer Freude die jeweils erschienenen Hefte und arbeite sie mit reichem Gewinn für meine wissenschaftliche Arbeit durch. Alle, die für sprachgeschichtliche Fragen Interesse haben und die auf diesem Gebiete thaetig sind, müssen Ihnen und Ihren Mitarbeitern innig dankbar sein für Alles, was Sie zur Bereicherung der Wissenschaft in selbstloser Arbeit beitragen, in kritischen Verarbeitung zugaenglich machen. Dass sie dabei meiner gedachten und mich durch Ihre Sendungen an Ihren Ergebnissen Theil nehmen lassen, das dank ich immer Ihnen ganz besonders.

Mit nochmals herzlichem Danke und den besten Wünschen für den Fortgang Ihrer Studien und Veroeffentlichungen bin ich mit besten Empfehlungen

Ihr ergebener

A. Wiedemann.

THE AMERICAN COUNCIL OF LEARNED SOCIETIES, under date of May 15, 1936, makes the following announcement of grants in aid of publication:

The American Council of Learned Societies is prepared to extend assistance in publishing a limited number of meritorious works in the field of the humanities written by American scholars. It invites its constituent societies to propose books they deem suitable for assistance, but reserves the right to consider works submitted by others if the Executive Committee has accepted them for consideration.

Works proposed for publication must be complete in themselves, preferably the results of constructive research presented in the form of volumes of conventional size. Important books of reference and critical editions of valuable texts may also be submitted. Applications for aid in the publication of doctoral dissertations will not be considered save in exceptional cases. It is expected that few dissertations, in the form in which they are presented in candidacy for the degree, will be approved by the committees of the constituent societies and by the jury.

Plans for the manufacture, publication, and distribution of each assisted work, and for the disposition of any proceeds, must be approved by the Executive Committee.

Applications for the next awards of grants in aid of publication, on forms provided for the purpose, must be received in the Executive Offices of the Council, 907 Fifteenth Street, N. W., Washington, D. C., on or before October 1, 1936. Applications must include descriptions and critical appraisals of the works proposed, together with full manufacturing specifications and estimates of cost. No work can be considered of which the manuscript is not available for examination in completed form.

MORGAN CALLAWAY JR., Professor of English at the University of Texas, and a member of the LINGUISTIC SOCIETY OF AMERICA since 1931, died at Austin, Texas, on April 2, 1936, after a long illness, in his seventy-fourth year.

He was born at Cuthbert, Ga., on November 3, 1862, and received the degrees of A.B. and A.M. from Emory College (Georgia) in 1881 and 1884, and that of Ph.D. from The Johns Hopkins University in 1889, where also he was elected to Phi Beta Kappa. He was Adjunct Professor of English at Emory College from 1881 to 1883, and Professor of English

at Southwestern University, Texas, from 1884 to 1886 and again 1889 to 1890, the interval being spent in graduate study. In 1890 he was called to the University of Texas as Assistant Professor of English, and he spent there the remainder of his life, becoming successively Adjunct Professor in 1891, Associate Professor in 1893, and Professor in 1898. In 1928 he was Vice-President of the Modern Language Association of America.

His special subject of scholarly interest was the syntax of Old English, in which he was a recognized authority. Among his more important publications are treatises on The Absolute Participle in Anglo-Saxon (1889), The Appositive Participle in Anglo-Saxon (1901), The Infinitive in Anglo-Saxon (1913), Studies in the Syntax of the Lindisfarne Gospels (1918), The Historic Study of the Mother Tongue (1925), and The Temporal Subjunctive in Old English (1931). His review in LANGUAGE 10.363–76, of Otto Jespersen's System of Grammar, will be remembered by members of the LINGUISTIC SOCIETY as a fundamental and penetrating treatment of the theme.

DR. FRANZ BOAS, Professor of Anthropology at Columbia University, has become Professor Emeritus.

DR. M. B. EMENEAU, of Yale University, who has been in India during the past year making research studies on the non-Indo-European languages of the region, as Fellow of the American Council of Learned Societies, will continue his work during the coming year under a grant from the American Philosophical Society.

PROF. CHARLES KNAPP, of Columbia University, has been elected President of the Classical Association of the Atlantic States, of which, since its foundation twenty-nine years ago, he has been Secretary and Treasurer, as well as Editor of its organ, The Classical Weekly. His retirement from these more onerous duties was caused by ill-health. As Secretary and Treasurer he has been succeeded by Dr. John Flagg Gummere, of the Penn Charter School, Philadelphia.

DR. HERMANN MEIER, Assistant Professor of German in Drew University, has been advanced to the rank of Associate Professor.

DR. MERLE M. ODGERS, Assistant Professor of Latin and Dean of the College of Liberal Arts for Women at the University of Pennsylvania, has become President of Girard College, where he took up his new duties on July 1.

DR. FRED N. ROBINSON, Professor of English at Harvard University, has been elected Gurney Professor of English; he succeeds George Lyman Kittredge, who retired after fifty years of service to the institution.

DR. EDGAR H. STURTEVANT, Professor of Linguistics at Yale University, has been elected President of the American Oriental Society for the year ending Easter Week, 1937.

DR. MORRIS SWADESH, of the Institute of Human Relations, Yale University, gave a course in English Phonetics in the summer session of the College of the City of New York.

DR. JAMES R. WARE, Lecturer on Chinese at Harvard University, has been promoted to an Assistant Professorship.

JOSHUA WHATMOUGH, Associate Professor of Comparative Philology at Harvard University, has been promoted to the grade of Professor of the same subject.

THE FOLLOWING NEW MEMBERS FOR 1936 were received into the LINGUISTIC SOCIETY subsequent to the last published list, and up to May 31, 1936:

C. Ernest Bazell, M.A., Research Fellow of Magdalen College, Oxford, England; *English philology.*

Moshé Berkooz, Ph.D., 29 Eliezer Ben-Yehuda St., Tel-Aviv, Palestine; *Semitics.*

Louis Hjelmslev, Ph.D., Docent in Comparative Linguistics, University of Aarhus; 8 Knud Rasmussensvej, Aarhus, Denmark.

Mrs. Leicester B. Holland (Louise Adams), Ph.D., 4203 Pine St., Philadelphia, Pa.; *pre-history of Italy.*

Russell Blaine Nye, M.A., Assistant in English, University of Wisconsin, Madison, Wis.; *early American pronunciation.*

Muhammad A. Simsar, D.C.S., 317 S. 41st St., Philadelphia, Pa.; *Persian, Turkish, Arabic.*

Hans Jörgen Uldall, M.A., Udby pr. Örsted, Denmark; *phonetics, American Indian languages.*

Camil A. J. Van Hulse, 1029 North Euclid St., Tucson, Arizona; *Indo-European.*

Frederick E. Wirth, A.M., Associate Professor of Ancient Languages, St. Bonaventure College, St. Bonaventure, N. Y.

BOOKS RECEIVED

Under this heading will be acknowledged such works as seem to bear on the advancement of the scientific study of language.

The publicity thus given is regarded as a full return for the presentation of the work. Under no circumstances is it possible to comply with the requests being made by certain publishers for the return of books not reviewed quickly.

Reviews will be published as circumstances permit. Copies of them will be sent to the publishers of the works reviewed.

For further bibliographic information consult the annual list of Exchanges.

Acta Philologica Scandinavica 10. 81–376 (1935–6).

Aegyptus 15. 385–440 (1935).

American Speech 10. 163–320; 11. 1–104 (1935–6).

Annals of the Bhandarkar Oriental Institute, Poona 16. 173–326 (1935).

Anthropos 30. 655–949; 31. 1–339 (1935–6).

L'Antiquité Classique 4. 293–554 (1935).

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Archiv für Orientforschung 10. 317–400 (1936).

Archiv Orientální 7. 277–532 (1935).

Arkiv för Nordisk Filologi 51. 205–326 (1935).

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Bolleti del Diccionari de la Llengua Catalana 17. 65–144 (1935).

Bolletino delle Pubblicazioni Italiane Nos. 409–414 (1935).

Bulletin de la Société de Linguistique de Paris 36. nos. 107–8 (1935).

Bulletin Hispanique 37. 417–544 (1935).

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The Bulletin of the Institute for Research in English Teaching Nos. 118–122 (1935–6).

Bulletin of the School of Oriental Studies (University of London) 8. 1–881 (1935–6).

Burhan's Tuzak-i-walajahi Part I. Pp. 147. By S. MUHAMMAD HUSAYN NAINAR. (Madras University Islamic Series No. 1.) Madras: 1934.

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Die Entstehung flektierenden Sprachen. Pp. 108. By CARL
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Teil. Zweiter Band: Germanisch I 185–455. By W. STREITBERG,
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Lund: C. W. K. Gleerups Förlag, 1935.

Groupe Linguistique d'Études Chamito-Semitiques. Comptes Ren-
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Hessen's Irish Lexicon. (do)ascnai (65)—cennaid (144). By
SÉAMUS CAOMHÁNACH, RUDOLF HERTZ, VERNAM E. HULL, GUSTAV
LEHMACHER. Halle (Saale): Max Niemeyer, 1935.

Harvard Journal of Asiatic Studies 1. 1–161 (1936).

Hesperis 20. 1–98; 21. 1–252 (1935).

Ἴαμβος: Aryan Sacred Voice of Stress. Pp. 70. By THOMAS
FITZHUGH. Charlottesville: Anderson Brothers, 1935.

Indledning til Tysk Grammatik. Pp. 78. By L. L. HAMMERICH.
Copenhagen: G. E. C. Gads Forlag, 1935.

Indogermanische Forschungen 53. 245–324; 54. 1–80 (1935–6).

Italica 12. 197–224; 13. 1–30 (1935–6).

The Jewish Quarterly Review 26. 199–422 (1936).

Journal de la Société des Américanistes 27. 1–261 (1935).

The Journal of English Philology 4. no. 3 (1935).

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THE CONFLICT OF HOMONYMS IN ENGLISH

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I

The studies of Gilliéron and his followers in linguistic geography have made us familiar with the doctrine that two words of different origin which become homonyms by regular sound-changes may interfere with one another to such an extent that one is ultimately excluded from the vocabulary of a given dialect.   The lack of a linguistic atlas of England has prevented the application of this principle, and likewise of others expounded by Gilliéron, to English dialects.   For it was the comparison of maps which enabled Gilliéron and Roques to show, for example, that only in those French dialects where *mulgere* would have become homonymous with *molere*, is it replaced by other words for 'milk', such as *tirer, traire, ajuster, aria, blechi*, etc.[1]   Although neat demonstration of this sort is seldom possible in English, conclusions about the conflict of homonyms may sometimes be reached, in the absence of an atlas, by more roundabout methods.   Enough information for reasonable deductions may be obtained from the combined materials of the English Dialect Dictionary, which gives the distribution of a large part of the vocabulary in modern dialects, and the New English Dictionary, which gives a detailed semantic history and the approximate time of a word's disappearance from the language.

The studies hitherto made of the subject have been based almost exclusively on the NED.   Several pupils of Holthausen have considered the development of homonyms among other causes of the loss of words, such as the disappearance of the object or idea itself and the substitution of words from other languages of different or superior culture.   More recently, Jaeschke, in a subtler analysis of the causes of obsolescence, included the conflict of homonyms, considering English words in the volumes of the NED (T-Z) which were not available to his

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[1] Jules Gilliéron and Mario Roques, Études de Géographie Linguistique 10–18 (Paris, 1912).

predecessors.[2]   In general, it may be said that most of those who have discussed the principle with reference to English have either rejected it too summarily, on the ground that it was disproved by the continued existence in English of such homonyms as *lie* 'prevaricate' and *lie* 'recline' and the two adjectives *light*;[3] or have applied it uncritically by assuming that when one of two homonyms was lost, it must have been lost because of homonymy.   This is a dangerous method in a language that has suffered so many changes in vocabulary as English.   There were, for example, four Old English words of the form *ār*: *ār*, masc. 'messenger', *ār* fem. 'honor', *ār* neut. 'bronze', 'copper', represented in

[2] Holthausen in Germanisch-romanische Monatschrifte 7.184-96, summarises the results of four dissertations by his students: Emil W. Hemken, Das Aussterben alter Substantiva im Verlaufe der englischen Sprachgeschichte, Kiel, 1906; Johannes R. W. Offe, Das Aussterben alter Verba und ihr Ersatz im Verlaufe der englischen Sprachgeschichte, etc., Kiel, 1908; Wilhelm Oberdörffer, Das Aussterben altenglischer Adjektive, etc., Kiel, 1908; Friedrich Teichert, Über das Aussterben alter Wörter im Verlaufe der englischen Sprachgeschichte, Kiel, 1912. Teichert is more doubtful of the importance of 'lautlicher Zusammenfall' (46-7) than the others, and Holthausen (196) apparently shares his skepticism.   Henry Bradley, whose work on the NED made him aware of certain conflicts, mentions the subject, On the Relations between Spoken and Written Language, 24-5 (Oxford, 1919); and Leonard Bloomfield, who treats several aspects of homonymy, gives some good English examples of interference, Language 396, 398 (New York, 1933).   Sir Robert Bridges, On English Homophones, Society for Pure English, Tract II, Oxford, 1919, is interesting, but unscientific; cf. Edwin B. Dike, 'Obsolete Words', JEGP 34.356-7.   Kurt Jaeschke's Beiträge zur Frage des Wortschwundes im Englischen, Sprache und Kultur der germanischen und romanischen Völker, Anglistische Reihe 6, Breslau, 1931, is the best treatment; but most examples are discussed in summary fashion.

That the validity of Gilliéron's principle has been generally accepted by students of French dialects is evident from Karl Jaberg's Sprachgeographie, Aarau, 1908, and the discussions in such handbooks as Albert Dauzat's La géographie linguistique, Paris, 1922, and Ernst Gamillscheg's Die Sprachgeographie und ihre Ergebnisse für die allgemeine Sprachwissenschaft, Neuphilologische Handbibliothek für die westeuropäischen Kulturen und Sprachen 2, Bielefeld and Leipzig, 1928.   For German dialects see Adolf Bach, Deutsche Mundartforschung, 108-10 (Heidelberg, 1934).   For criticism of the hypothesis and a not very successful attempt to explain the process of elimination in homonymic conflicts, see Elise Richter, Über Homonymie, Festschrift für . . . Paul Kretschmer 167-201 (Wien-Leipzig, 1926).   For Latin examples, see Roland G. Kent, "No Trespass" in Latin Linguistics, Classical Studies in Honor of John C. Rolfe 143-61, (Philadelphia, 1931).   B. Trnka's interesting Bemerkungen zur Homonymie, Travaux du Cercle Linguistique de Prague 4.152-6, I had not seen when this article was written, but I comment on it in two notes below.

[3] Cf. below (242).

modern *ore*, and *ār* masc. 'oar'.   The first two words no longer exist in Modern English.   Now if we examine the number of words that have disappeared from the Old English vocabulary without any possible influence of homonymy, we find that it is well over fifty per cent.   Thus of any four words, homonyms or not homonyms, two might easily have disappeared for other reasons.   Actually, in the case of *ār*, there would be little reason to suppose, as does Hemken,[4] that a noun denoting a person would be confused with an abstract noun, and neither is likely to succumb to the competition of two nouns denoting objects.

If one of two words that became or would have become homonyms has been lost, the loss should not be attributed to this fact without observing several rules of caution.   The first is that two homonyms are unlikely to interfere unless they belong to the same part of speech: a verb is unlikely to conflict with a noun or a noun with an adjective.   It is a priori improbable that OE *earm*, 'poor', 'wretched', disappeared because of *earm*, 'arm'.   Usually the homonyms must also have the same syntactic function, and be capable of being used in the same construction. *Cleave* 'sunder' (OE *clēofan*) is usually transitive and *cleave* 'adhere' (OE *cleofian*, *clifian*) intransitive, but even when the former is used intransitively, as it sometimes is, it is distinguished from the second verb by the fact that the latter is always accompanied by a preposition, in Modern English (*un*)*to*.   A second condition is that the words must fall within the same sphere of ideas and be likely to appear in similar contexts.   An exception to this rule is the case of a word that would be avoided because its homonym was vulgar or in some way ludicrous; the avoidance of *ass*, because of the popular pronunciation of *arse*, cited by Bloomfield,[5] would be a case in point.

When these two conditions are fulfilled, it is possible for a combination of sounds representing two different words to become embarrassingly ambiguous, and the resultant confusion may be so marked as to lead to the elimination of one of the words.   But even when a homonym has been lost under these conditions, it is hard to prove that homonymy was the determining cause.   It is obvious that other and more usual causes of obsolescence must first be ruled out; and this is not easy because they are often obscure and elusive.   Some indication of the probabilities may be obtained by a comparison of the chronology of sound-changes and the time of the disappearance of a word.   If a word is lost or begins to be less frequently used after the sound-change result-

[4] Page 30.
[5] Language 396.

ing in homonymy takes place, this may confirm a suspicion that phonetic identity was a factor involved.   Conversely, if one of two words which would have become homonymous in Modern English is proved by the NED's records to have disappeared as early as Middle English, the influence of homonymy is automatically eliminated.   When all these cautions are observed, the influence of homonyms on the loss of words may sometimes be reasonably inferred.   Actual proof, however, is possible only when it can be shown that a word has disappeared in dialects where by normal sound-changes it became identical with another, but has been preserved in dialects where by normal development the two words remained different.

An illustration of the importance of consulting dialects may be seen in the homonyms *queen, quean*.[6]   Modern English *queen* and *quean* (archaic, obsolescent) are, of course, two different words, though ultimately related by ablaut: OE *cwēn*, 'queen', 'princess'; and OE *cwene* 'woman', 'servant', 'harlot'.   In Middle English, in spite of the lengthening of the short *e* in open syllables to [ɛ:], the two words are still easily distinguishable because close and open long *e* remain two different phonemes; the author of Piers Plowman can use the two words in the same line (C 9.45).   A vocalic distinction was still maintained in the sixteenth century, when [e:] had been raised to [i:] and [ɛ:] to [e:]; and the word *quean* is still familiar to us from Shakespeare.   But when, around 1700, the [e:] of *quean* (ME [ɛ:]) also became raised to [i:], it proved impossible for the same sounds [kwi:n] to continue to represent both a royal personage and the commonest kind of woman, and the word *quean* disappears from spoken Standard English.   It might be suspected that *quean* 'low woman', 'harlot' is the kind of word which would be subject to replacement in any case because of euphemism.   But a survey of the distribution of *quean* in English dialects clearly corroborates the view that confusion with *queen* is the cause of its disappearance.   *Quean*, according to Wright's Dialect Dictionary, does not occur in the larger part of the Midlands, especially the East Midlands, and the Southeast, the territory in which ME [e:] and [ɛ:] fell together as they did in Standard English.   In a Southwestern area which includes Somerset and Devon the two Middle English sounds remain distinct;[7] and, according to Wright, we find that *quean* 'low woman', 'harlot' is

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[6] Bradley 24, said that 'when *queen* and *quean* came to be pronounced alike, it was inevitable that the latter should become obsolete as a spoken word.'   He was considering only Standard English.

[7] Karl Luick, Hist. Gram. §497.2.

still a living word in Somerset and Devon.   The evidence in the North, where initial *wh-* is often substituted for *qu-* in *quean*, and in Scotland, where *quean* has lost its evil meaning, is too complicated for presentation here, but it confirms the other evidence.[8]

When dialectal variants are not available, the case for the conflict of homonyms is less clear; but it may sometimes be reasonably inferred from the history of words in Standard English.   In Middle English there were three verbs of the form *aleg(g)e* and three of the form *alaye*. Their interactions may be summarized in Table 1 on the basis of the NED's material.

Alegge[3] would probably have disappeared of itself, just as *lay* and *say*

TABLE 1

Etymology	ME Form	ME Meaning
VL *exlitigare*(?) > OF *esligier*, infl. by AN *alegier* < *allegare*	ALEGE[1]	plead, declare
Lat. *alleviare* > OF *aleg(i)er*	*alege*[2]	alleviate
OE *alecgan*	*alegge*[3] ⎫	put down, re-
OE *alegð*, 3 sg.pres.ind. of above	ALAYE[1] ⎭	press
Lat. *allegare* > OF *aleier*	*alaye*[2]	allege, declare
Lat. *alligare* > ONF *aleier*	*alaye*[3]	alloy, temper
Lat. *alligare* > OCF *aloier* > Mod F *aloyer*	[*alloy*]	

triumphed in late Middle English over the less common forms *legge* and *segge* from the infinitive.   Before *alegge*[3] disappeared, both the forms *alegge* and *alaye*, from different parts of OE *alecgan* and apparently interchangeable in meaning, took over the meaning of *alege*[2], 'alleviate', which is thus preserved in modern *allay*.   Then both *alegge*[3] and *alege*[2] went out of use, the latter in the sixteenth century probably because of conflict with *alege*[1].   *Alaye*[2] disappeared (last quoted in 1470), chiefly because of the triumph of the common legal form *allege*, but partly because of confusion with *alaye*[1] and *alaye*[3].   *Alaye*[1] and *alaye*[3] persisted side by side, though sometimes confused, as the NED shows, because of such a figurative meaning of *alaye*[3] as 'temper', (last quota-

[8] Edna R. Williams in an unpublished dissertation on the conflict of homonyms now provides maps showing the development of *quean-queen* in various dialects, and discusses the interference of the two words more fully.

tion 1769) which was very similar to that of *alaye*[1], until *alaye*[3] was replaced in the nineteenth century by *alloy* from Modern French. One can hardly look at the fate of these words without suspecting that phonetic identity played a part in eliminating some of them and left us with distinct forms for different ideas. Only one of the three verbs *alege* survives from Middle English, and only one of the three verbs *alaye*.[9] The survivors are indicated by small capitals in the table.

II

It would not be difficult to multiply examples of the types just given; but my present purpose is not so much to demonstrate the validity of Gilliéron's principle by illustration from English as to suggest various possible applications of the principle which students of English have neglected.

The relation of homonymic interference to the borrowing of French words in English is worth considering. The very abundance of French loan-words and the great variety of causes which brought about their introduction[10] make it difficult to be sure of any real conflict among native homonyms that have been lost. Still, when the influence of law, war, religion, cuisine, and other fields in which the political and social dominance of the Normans facilitated borrowing, is not apparent, the reasons for the replacement of common English words by French is often hard to find. Jespersen has a list of common words for the introduction of which he finds no other explanation than that it was considered fashionable to interlard one's speech with French: this includes such words as *cover, cry, turn, use*.[11] Plainly fashion often played a part in the elimination of native words, but it seems quite possible that homonymy at least aided the substitution of French and Latin words for English in some of these cases. Take *cover*. The three commonest words for 'cover' in Old English were *wrēon, helan* and *ðeccan. Ðeccan*

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[9] I am not here considering how far there was consciousness of separate words in these cases. *Alaye*[1] and *alaye*[3] were apparently sometimes apprehended as two meanings of one word in the eighteenth century. The difference between the lexicographer's and etymologist's conception of the word and that of the ordinary speaker and hearer has, of course, an important bearing on the interference of homonyms, and is touched on below (243–4).

[10] On this subject see Otto Jespersen's Growth and Structure of the English Language, Chap. V; Robert Feist, Studien zur Rezeption des französischen Wortschatzes im Mittelenglischen, Leipzig, 1934, Beitr. zur engl. Philologie 25; Mary S. Serjeantson, Foreign Words in English, London, 1935, Chap. V.

[11] Growth and Structure[4] 91–2.

became limited in early Middle English to a specialized sense of 'cover', i.e. 'thatch', which it was already developing in Old English. *Wrēon* may have been started on its decline by the fact that it was a contract verb, all but the commonest of which have perished. Still, it was used in Middle English, and occasionally survives in modern dialects; but in Middle English it attained the same form as another verb with a variety of meanings, OE *wrīgian*, intr. 'proceed', tr. 'twist', 'turn aside', etc. (the two are discussed in the NED under *wry* v.¹, and *wry* v.²). Other words for 'cover', both the strong *helan* and the weak *helian*, 'conceal by covering', likewise fell in with a verb and a very important one *hǣlan* 'heal'.[12] *Hele* is still used in gardeners' language of covering plants, but is no longer an ordinary word for 'cover'; the semantic fullness of ME [hɛːlə] must have proved inconvenient and facilitated the introduction and spread of *cover*, which first appears around 1300.

It is possible for words of French origin, after they have established themselves in the language, to interfere with native words, either because the French loan-word is first used in a specialized sense and only later comes into common use, or, more often, because, although differentiated from the native word at the time of its introduction, it becomes identical with the latter through phonological processes affecting one or the other.[13] Even a slight difference in the endings of two words may be sufficient to prevent confusion for a time. Thus, the commonest native word for 'use' in early Middle English was *notie* (OE *notian*) which, in the present forms with *-ie* and preterite forms in *-ede*, was distinguished from *note* (pret. *noted*) 'mark', 'observe', from OF *noter*, Lat. *notare*. But when both the differentiating *i* of *notie* and the final *e* of *notede* disappeared in the fourteenth century (earlier in the North), *note* 'use' soon disappeared, while *note* 'mark', 'observe' which had now become a popular word, remained. *Use*, another French word, which was beginning to establish itself in the language in other senses now replaces *note* 'make use of' in this sense,[14] NED's earliest quotation for the meaning 'make use of' being dated 1303. OE *notian*, ME *notie*,

[12] Cf. Offe 25.

[13] An illustration of the elimination of a French loan-word, once well established in the language, by a native word which became homonymous with it is the case of archaic *strait* (OF *estreit*) and *straight* (OE *streht*), discussed by Miss Williams. I do not agree with Trnka's contention, Bemerkungen 153, that foreign words in English occupy a peculiar position and do not conflict with homonymous words of native origin.

[14] Cf. Teichert, 49.

'note' was not the only native word for 'use', 'enjoy'. The related *nyttan* had, however, died out around 1250, probably because the descendants of *notian* were protected by the corresponding noun OE *notu*, ME *note* 'benefit', 'use', 'enjoyment'. Still another word, OE *brūcan*, 'enjoy', 'use', survived in Middle English, but except in the Northern dialect and Scottish, where it is still found today, it became limited in early Middle English to the specialized sense of 'make use of (food)'.[15] Thus neither the descendant of OE *nyttan*, nor that of *brūcan* was in a position to become the ordinary word for 'make use of' at the beginning of the fourteenth century. Although proof is hardly possible in such a case, the combination of circumstances makes one suspect that the homonymy of the native and foreign *note* was one of the factors in establishing *use* as one of the commonest words in our language.

III

The disappearance of certain Old English forms of the personal pronoun provides a good illustration of homophonic conflict, because obscure preferences are much less likely to operate in the case of mere substitutes than in obsolescence of nouns, verbs, and adjectives. Many curious and difficult forms replace the normal descendants of the Old English pronouns. We might expect the development of a variety of forms due to lack of stress; but we find, in addition, the substitution of rare, sporadic forms for those which would have been regular by phonetic law, the occasional adoption of the demonstrative pronoun as the personal, and even numerous borrowings from the pronouns of a foreign language.

Thus, beside such forms for 'she' as *he*, which is a rare form of the Northeast Midland, *heo, ho* [hø:] in the West Midland, *hue* in the Southwest, and *hi* in Kentish, all of which might be considered regular dialectal developments from OE *hēo* or *hīo*, we have *ȝo, sho,* and *she*. *ȝo* [ço:] may be explained as a development of *hīó* with a rising diphthong instead of the normal *hío, héo*; but the origin of *sho* and *she* has been much disputed, the older scholars attempting to derive them from the demonstrative *sēo*, and later students, with more probability, explaining *sho* by the combination of the third singular present indicative ending

[15] Both the weak inflection and the phonology of *brook* in ME point to the existence of an OE *brucian*, beside *brūcan*. It is barely possible that the identity of the past participle *brocen* of the strong verb with that of the common verb *brecan*, 'break', helped to establish the weak verb. The NED finds no certain instance of either the strong preterite or past participle in ME.

-*s* with descendants of *hīo*-forms, as in the inverted order *telles 30.
She* would then be a blend of *sho* and *he*.[16]   The important point for us
is that *sho* and *she* become the regular pronominal forms in the North
and Northeast Midland, where the nominative singular masculine and
the nominative singular feminine would have become identical in the
twelfth century; OE *hēo* here became *he* because of the unrounding
of [ø:] (<OE *ēo*) to [e:][17]   In the West Midland and Southwest, where
the representatives of OE *ēo* did not become [e:] but remained rounded
until the fourteenth century or later, *h*-forms persist throughout the
Middle English period, *hue* being a common spelling in the Southwest
and *ho* the regular form in the West Midland and Northwest Midland,
where it persists in some counties even in modern dialects as [hu:].
Here the fact that the vowel differentiated the form from the masculine
he made the introduction of *sho* and *she* unnecessary.   Where the cus-

TABLE 2

WEST SAXON PLURAL PRONOUNS

	9th Cent.	10th Cent.	11th Cent.	12th Cent.
G.	*hiera, hiora, heora*	*hiera, hiora, heora*	*hira, hyra, heora, hiora*	*heora, hyra, hera, here*
D.	*him*	*him,* rarely, *heom*	*him, heom*	*heom, hem, him, hym*

tomary distinction between masculine and feminine was lost, Middle
English made use of what would otherwise have remained mere nonce-
forms or sporadic variants in order to maintain it.

Since I hope to treat other clashes among the pronouns more elabo-
rately elsewhere, I will give only one other example here.   The dative
singular masculine and dative plural had in the earliest Old English texts,

[16] Martin B. Ruud, MLN 35.222–5; Harald Lindqvist, Anglia 45.1–50.   NED,
which adheres to the derivation of *she* from *sēo*, notes that the phonetic develop-
ment which made *hē* and *hēo* undistinguishable in some dialects furnished 'a strong
motive for using the unambiguous feminine demonstrative', but does not adduce
the dialectal evidence given above for the influence of homonymy.

[17] Where the ONth. had *hīo*, this would, presumably, if the original diphthongal
stress were preserved, become LOE *hēo*, provided *īo* here developed as did the *īo*
from *i*-umlaut of *īu*, cf. Richard Jordan, Handbuch der mittelenglischen Gram-
matik §86.

because of the loss of endings, both attained the form *him*. It is interesting to observe the beginnings of an attempt at differentiation in Old English, and to find distinctive forms everywhere developed in Middle English. The easiest method of illustrating the gradual shift from *him*, plural, to the analogical *heom* will be to cite in Table 2 the West Saxon plural pronominal forms given by Bernhard Gericke.[18] I give the genitive forms because of their influence on the dative. *Heom* appears as a rare form in the tenth century, the diphthong arising by analogy with that of the genitive plural. By the eleventh century it has become common, and by the twelfth *heom* and the resultant *hem* have relegated the ambiguous *him* to the position of a secondary form. In Mercian the same development took place, though the paucity of early texts makes it impossible to follow the shift so clearly. In Middle English the descendants of *heom*, i.e. *hem*, *hom*, and *ham*, are the normal forms in the Midlands and Southwest. In the North *him* was still the plural form in the tenth century. At the beginning of the Middle English period, it has been replaced by *þaim* from the Old Norse. The ambiguous *him* has thus been everywhere ruled out.

It might be objected that the identity of pronominal forms could hardly be confusing, because the very fact that they must refer to persons previously mentioned would indicate clearly whether a pronoun was masculine or feminine, singular or plural. This theoretical objection, however, applies only to a carefully cultivated literary style. Both the difficulties that sometimes arise in interpreting *him* in Old English texts and the slovenly use of pronouns in popular spoken English indicate that there might occasionally be sufficient reason to prefer a distinctive form to an ambiguous one. Over a long period of time the occasional necessity of choosing a distinctive variant could easily lead to the establishment of the originally abnormal and infrequent form as the regular one. It is true that one may point to instances in several languages of the abandonment of a distinctive pronoun, especially when that of one person or number is transferred to another. The fact that Modern German has adopted the pronoun of the third person plural as the customary pronoun of the second person singular and plural shows that fashion and etiquette may sometimes triumph over ambiguity; but this, I believe, is a phenomenon that would be characteristic only of a language of culture consciously developed. Actually, as Prokosch points out to me, such a sentence as 'Ich habe [zi:] gesehen', with its

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[18] Das Personalpronomen der 3. Person in spätags.- und frühmittelenglischen Texten, Palaestra 193.85–6 (Leipzig, 1934).

three possible meanings, causes awkward ambiguities in spoken German. In any case, these possible objections to accepting the influence of homonymy in the choice of English pronominal forms seem to be outweighed by the striking historical evidence of Old and Middle English dialects. It is hard to find any other reason for the elimination of normal forms and the selection of various by-forms and other substitutes than the development of phonetic identity.

IV

A notable difference exists between the East Midland and the West Midland texts in forms of the preterite of the fourth and fifth ablaut classes. The East Midland dialects developed early *o* and *a*-forms in the stem of the plural: the *a*-forms are probably from the singular, the *o*-forms are a little difficult to account for, but may have arisen from some analogy with the past participle of class IV. These two types

TABLE 3

		Present Plural	Preterite Plural
E Midland	OE	*berað*	*bæron*
	ME	*b[ɛ:]re(n)*	*b[ɛ:]re(n)* replaced by *bore(n)*, *bare(n)*
W Midland	OE	*berað*	*bēron*
	ME	*b[ɛ:]re(n)*	*b[e:]re(n)*

gradually tend to replace the early *e*-forms which would have been the regular development of OE *bæron* (*bēron*), *spæcon* (*spēcon*).[19] In the West Midland, however, the usual vowel of the preterite plural is *ē*, as in *spēken*, *bēren*, and such texts as The Pearl and The Chester Plays show by their rhymes that this is close *ē* [e:] from Anglian *bēron*, *spēcon*,[20] as we should expect. The analogical relationships in these classes are, of course, complicated, but no one has pointed out that if the East Midland texts had regularly preserved the Old English preterite plural vowel, the

[19] James F. Rettger, The Development of Ablaut in the Strong Verbs of the East Midland Dialects of Middle English, Language Dissertations, Ling. Soc. Amer. Chaps. III, IV, VIII, esp. 88 (Baltimore, 1934).

[20] Samuel O. Andrew, 'The Preterite in Northwestern Dialects', Review of English Studies 5.431–6. Though Andrew did not examine carefully the West Midland texts proper, which are mostly late, as opposed to the Northwest Midland, he cites *e*-forms from the early Katherine group.

preterite plural would have been exactly the same as the present plural, i.e. *bēren, spēken* with [ɛ:], since the Midland endings are likewise the, same.   For, as the evidence of place-name shows, open *ē* [ɛ:] from OE *ǣ* not close *ē* [e:] from OE *ē*, is characteristic of East Anglia, as it is of Saxon territory, and even Orm in Lincolnshire has predominantly *æ*, indicating open *ē*.   Table 3 shows the development in condensed form. In West Midland the close *ē* (OE *ē*) differentiated the preterite from the present.   Here the characteristic *e*-vowel of the plural is even adopted in the singular, replacing *a*, and *bēr* becomes the usual preterite form for the singular.   The contrast between the East Midland, where the regular phonological development of the plural is generally replaced, and the West Midland, where it is preserved, must be partly due to the fact that the present and preterite plural would have been identical in the East and different in the West.[21]

Homophonic influence on the preterite forms of weak verbs may be seen in the case of verbs of the type OE *sende* (pres. 1 sg.), *sende* (pret. 1 sg.), which show two kinds of differentiation by Middle English times: *sēnde, sende*; and *sende, sente*.   Bülbring pointed out that in Robert of Gloucester the rhymes in *-ende* proved conclusively that the vowel of the preterite and past participle was short, while that of the present and infinitive was long: *sēnde, sende*.[22]   That is, the Old English ninth-century lengthening before *-nd* either did not spread to the preterite, or a short vowel was reintroduced after lengthening had taken place.   Bülbring attributed the differentiation to the analogical influence of verbs like *fēdan, fĕdde*, which normally shortened the *ē* of the preterite because of the double *d*; but it seems reasonable to suppose that the phonetic identity of the first and second persons singular of the present indicative (ending in *-e, -est*), and that of the whole present and

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[21] It may be noted that this explanation is somewhat similar to the principle of contrast expounded by Eduard Prokosch, JEGP 20.468-90, for the Germanic verb in older periods.   A more exact parallel is the explanation of the OF preterite *valui* given by Friedrich Schürr, Zeitschrift für romanische Philologie 41.139-41; it replaces the **vail* which would have been identical with the present; cf. Gamillscheg, Die Sprachgeographie 45, and for a survey of the controversy concerning this and other forms, Über den Schwund des einfachen Praeteritums, Donum Natalicium Schrijnen 86-8 (Nijmegen-Utrecht, 1929).

[22] Englische Studien 20.149-54.   The present and infinitive rhyme only with themselves and words of the type *hende, ende*, whereas the preterites rhyme only with themselves and French words in *-ende*, which never underwent OE lengthening.   The difference was not inherited from the period when the preterite had *-dd-*.

preterite subjunctive facilitated the regularization of these analogical forms.[23]

Similarly, though Albert H. Marckwardt's careful study now shows clearly that homophony cannot explain the origin of the preterite form in -ente,[24] as had been suggested by Koch and Skeat, it is probable that the spread of such forms was to some extent governed by the factor of homophony.   Marckwardt admits that this serves to indicate a condition which might have led to a preservation and perhaps an extension of the -te forms after they did originate.[25]   It seems possible that some of the anomalies mentioned by him may be explained by the influence of homophony.   He notes, for example, that send, although one of the first verbs to adopt the irregular weak inflection, seems to manifest in the texts of the fourteenth century, and particularly in the North, a tendency to return to the older inflection.[26]   May it not be that the North abandoned the sent forms because in the Northern dialect the -s endings of the present indicative singular and plural made a differentiation of the tenses by other means unnecessary?   To settle this problem satisfactorily one would, of course, have to make a detailed study of the texts involved.   At any rate, it is plain that both in the strong and the weak Middle English preterites the factor of homophony cannot be disregarded even when analogy may explain the origin of forms produced.[27]

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Another aspect of homonymy which deserves investigation is the kind of semantic interference that does not develop to the point of excluding

[23] It must be admitted, however, that a similar analogical short vowel occasionally appears without this predisposing condition in cases where the verbal forms were already distinguished by the dental ending, as in hēren-hĕrde (the rd not causing shortening), as is shown by Orm's herrde, ferrde.

[24] Origin and Extension of the Voiceless Preterit and the Past Participle Inflection of the Irregular Weak Verb Conjugation, Essays and Studies in English and Comparative Literature, Univ. of Mich. Publ. in Lang. and Lit. 13.152–328 (Ann Arbor, 1935).   Marckwardt finds that these are analogical forms first appearing in the Southeast.

[25] Page 160, note 21.

[26] Page 313.

[27] B. Trnka explained the modern limitation of the idiom use to to the present tense by the development of homophony with the preterite, On the Syntax of the English Verb from Caxton to Dryden, Travaux du Cercle Linguistique de Prague 3.36 (Prague, 1930).   For the Middle English and Early Modern English I use to go we are now obliged to substitute such periphrases as 'I usually go', 'I am in the habit of going', 'I am accustomed to go'.

one homonym from the language, but results only in a limitation of meaning or induces a new division of meanings in the homonyms involved.   This possibility is seldom mentioned, presumably because proof of such an influence is difficult.   A pair like the two adjectives *light* in English (German *licht* and *leicht*), which became homonyms in Old English (both LOE *lēht, līht*) and have existed as such in the language for over a thousand years, is sometimes cited as proof of the fallacy of Gilliéron's theory.   Closer examination shows that their history, far from demonstrating the unlikelihood of homonymic confusion, really reveals its subtle operation in the gradual restriction of the meaning of one homonym to its less ambiguous senses.   In Old English, *light* 'lucidus' had a much greater variety of meaning than at present: it often meant 'bright', 'brilliant', 'shining', and could be used of a bright fire and bright eyes.   The author of Beowulf could speak of a *lēohtan sweorde*, but Shakespeare, if he desires to express the same idea must write 'put up your bright swords'.   In Modern German the territory covered by *licht* is much more extensive than in Modern English.   We cannot speak of a light fire, meaning 'ein lichter Feuer' (Goethe), nor use *light* in the sense of 'clear', 'lucid', as in 'diese Sätze sind mir nicht licht genug' (Kant), because of the co-existence of *light* 'levis'. Not only has *light* 'lucidus' suffered a considerable restriction of meaning because of *light* 'levis', but in contexts where the two are still likely to come into conflict, as in reference to materials, we continually resort to compounds to avoid ambiguity.   Thus tailors speak of light-weight materials, and a man advertises 'a light-coloured gray overcoat' (NED's quotation), where of course 'light gray overcoat' would be ambiguous. It is possible that *light* as applied to colors, 'pale', 'not dark', and *light* 'not heavy', are sometimes felt, by a kind of synesthesia, to be merely different meanings of the same word.   *Light* in 'a light cloud' is presumably the antonym of *heavy*, but might easily be that of *dark*.   This feeling has probably made it easier for the two homonyms to exist side by side.   Certainly *light* 'lucidus' has become, since Old English times, less varied in meaning, less intense, and one might almost say 'less heavy', because it has been subjected to the influence of its homonym.[28]

[28] It is perhaps worth noting that there were two OE verbs *līhtan*, 'to illuminate' and 'to make less heavy', and two verbs *lighten*, recorded from ME times. Though *lighten* 'illuminate' is still familiar to us from literature, as in 'lighten our darkness, O Lord', we tend to reserve *light* in transitive use, for 'illuminate' and *lighten* for 'relieve', though, occasionally, when *lighten* is unambiguous, we may say 'this wall-paper will lighten the room somewhat'.

The interaction of the two adjectives *light* shows how intimately the problem of homonymic conflict is connected with the problem of the limits of the word.   The lexical distinctions of the etymologist may not exist for the majority of people, who have no knowledge of a word's history.   As Bloomfield points out,[29] *ear* of corn (OE *ēar*, Gmc. **aχuz*), has become a marginal (transferred) meaning of *ear* (OE *ēare*, Gmc. **auzō*).   Most people who reflect on matters of language would probably feel that *ear*, an appendage to the stalk of corn, is a metaphorical transference from the *ear* of the body, just as is *head* of cabbage from *head* of the body.   Here, then, we have an instance of the merging of two words into one from the point of view of the ordinary speaker; if, indeed, he reflects on the subject at all.   Real confusion can hardly arise in the case of two words of this sort which are not used in the same context. But conversely, it is just as likely that one word may develop such different meanings as to seem to a person who is not an etymologist two or more different words, as in the classic instances *metal-mettle*, *flower-flour*, where we have marked the differentiation by the spelling.   Often the development of a multiplicity of meanings or of sharply divergent meanings reaches such a point that some have to be abandoned.   Students of the obsolescence of words have noted that this semantic 'plethora' is one cause of the disappearance of a meaning or of the word itself.[30]   Thus the word *owe* (OE *āgan*) meant in earlier English 'own', 'possess', but, as the NED points out, the increasingly important legal and commercial sense of *owe* 'to have a thing to pay' brought about the abandonment of its old meaning, for which *own, possess, have* are now substituted.   Holthausen believes that OE *ār* may have disappeared because it had come to mean too many things: honor, dignity, glory, reverence, mercy, favor, benefit, prosperity, revenue.   It seems to me inconsistent to accept this kind of explanation for obsolescence and reject the hypothesis of the interference of homonyms.   From the point of view of the speaker ignorant of origins, the embarrassment and confusion which is caused by multiplicity of meanings is likely to be as great when a form represents two or more etymologically distinct words as when it represents one.   Most students of homonyms and most semanticists pay little attention to this fact, but Jespersen pertinently remarks that 'the psychological effect of those cases of polysemy, where "one and the same word" has many meanings, is exactly the same as

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[29] Language 436.
[30] Teichert 40 ff.; Holthausen 195.

that of cases where two or three words of different origin have acci-
dentally become homophones'.[31]   Because of this relationship, the
conflict of homonyms should not be considered a merely curious and
abnormal phenomenon, differing from other linguistic processes.   The
study of homonymic interference involves the whole problem of the
word as an entity and illustrates some fundamental principles of
semantics.

[31] 'Monosyllabism in English', Proceedings of the British Academy 14.28
(1928); cf. Jaeschke 60: 'Der Gleichklang ist gewissermassen das Gegenstück zur
Mehrdeutigkeit.'

ASSIMILATION AND DISSIMILATION[1]

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[This article is summarized in its last paragraph.]

Linguistic science is still based on the formulation that phonetic change is regular and invariable, provided the conditions in which the sounds stand are the same. The moderate chorus of dissent in recent years has come largely from the dialect-geographers, who have found variations which to them indicate the invalidity of the formulation; but in reality their findings demonstrate rather that the differences of environmental conditions are unexpectedly complex in character: dialectal areas are unexpectedly small, social dialects are found to exist, dialect mixtures are omnipresent, especially where the movement of population is free.[2]

But this is here only an introductory aside: it serves to announce that I believe in the regularity of phonetic change, and to introduce the topic of my address: certain semi-regular modifying influences upon the operation of sound-changes. For among the numerous influences which produce 'exceptions' to regularity of phonetic change, some operate with such uniformity themselves that they may be considered only sub-formulas to the phonetic formula in question. There are however other types of 'exceptions' which seem sometimes to be regular and sometimes to be sporadic; such, for example, are assimilation and dissimilation. In the domain of each of these phenomena some formulas, or sub-formulas, can be set up which operate with regularity, while other examples are but sporadic manifestations. This is not unnatural: for these phenomena are the product largely of psychological conditions rather than of merely physiological, i.e., muscular, readjustments. Now the speaker's thoughts are inevitably somewhat ahead of his actual utterance, unless he is speaking with undue slowness and hesitation. It is

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[1] An address delivered at the Linguistic Institute, Ann Arbor, Michigan, on July 24, 1936.

[2] Cf. especially Eduard Hermann, Lautgesetz und Analogie, and Leonard Bloomfield's review of the same, LANG. 8.220–33.

this gap which interests me: the gap between the thought and the utterance of the words which express it.   And notably, it is the effect on the utterance which this gap produces, that is my special interest tonight. For in the struggle of the articulating organs to keep up with the mental activities, there is the possibility of alteration in the sounds uttered, when they leap ahead to some phoneme or syllable which is about to come.   In other words, an assimilation is produced by the replacement of some phoneme or phonemes by other phoneme or phonemes shortly to be uttered, in an environment more or less the same.   Inasmuch as we name the direction of assimilation (and of dissimilation) by starting from that which is permanent, assimilation is normally regressive.

Any examination of examples of phonetic assimilation will show the correctness of this formulation.   Thus in Latin *barba* came from **farbā*, *bibō* from **pibō*, *quīnque* from **penqᵘe*, *quercus* from **perqᵘos*, *coquō* from **quequō* from **peqᵘō*.   In Sanskrit an initial *s* was assimilated to an *ś* or *ṣ* which followed the first vowel of the word:[3] *śvaśuras* 'father-in-law', from **sᵤeḱuros* (seen in Greek ἑκυρός, Latin *socer*, Gothic *swaíhra*); cf. also *ṣaṣ-* 'six' from **seḱs* (Latin *sex*, Gothic *saíhs*).   The same regressive assimilations are seen in the Lithuanian words corresponding to the Sanskrit words which have been quoted: *šēšuras* 'father-in-law', *šešì* 'six'.   In numeral words, it is noticeable that most changes by the influence of other numerals are changes of the word of lower numerical value: thus Gothic *fidwōr fimf*, Eng. *four five*, where *four* has got its initial *f* from *five*; cf. Skt. *catvāras pañca*, with different initial consonants, representing a difference in the Prim. IE.   Also, Latin **noven* (cf. *nōnus* 'ninth', from **n̥oᵤenos*) became *novem* by the influence of the final of *decem*; Lith. *devynì* 'nine' got its initial *d* from *dẽšimt* 'ten'. For, in counting, the influence of the coming numeral was strong upon that which was being uttered; examples of the progressive influence, though occurrent, are very few in numerals, and may usually be seen to contain some additional factor which exerts an influence.

In consonantal clusters it is common to find that the posterior consonant exerts an assimilative influence upon the preceding: for example, in most languages a nasal is assimilated to the position of the following consonant, and a stop causes a preceding stop or sibilant to assume its own voiced or voiceless quality, as the case may be.   Thus Latin

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[3] In fact the only words in Sanskrit beginning with *saś-* or the same combination of consonants with another vowel, are compounds in which the first element is the prefix *sa-* or the prefix *su-*, and vriddhi-derivatives of the same; in which, of course, a feeling for the prefix will keep the initial sibilant unchanged.

*kom-dō and *in-pōnō became condō and impōnō, *ad-terō *ad-cadit became attcrō accidit, scrībo and regō have the participles scrīptus and rēctus.   English have in have done and have to becomes hav and haf, respectively.

Yet in clusters the regressive direction is not so uniform as when there is separation of the sounds concerned.   Thus, in Latin, the clusters ln, ld, ls, and probably lẹ all developed to ll; rs became rr.   In Oscan and Umbrian md and mb became respectively nn and mm.   Such changes can be matched in most languages[4]; yet the direction of assimilation even in consonantal clusters is predominantly regressive.   At the same time, it is clear that clusters present a somewhat different problem from that of sounds which are not in contact with one another, and their behavior cannot be taken as nullifying the general principle which is being upheld in this paper.

The influence of vowels upon contiguous consonants is much more commonly exerted upon preceding than upon following consonants.[5] If with the vowels we may include the semivowels, we note here the Balto-Slavonic palatalizations, the Aryan palatalization, the split of the original labio-velars in Greek, the spirantizing of Latin c (that is, [k]) as it develops into the Romance languages, etc.   Similar influence of vowels upon vowels, in the regressive direction, is seen in the Germanic umlaut phenomena, in the limited assimilations of vowels in ancient Greek, in the epenthesis seen in Avestan and in Greek.   In Modern Greek a front vowel makes preceding χ and γ weak palatal spirants, a back vowel makes them velar:[6] the palatal spirant in ὄχι 'no' and λέγει 'says', the velar in ἔχω 'I have' and ἐγώ 'I'.

The contrary change, Dissimilation, shows similar tendencies.[7]

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[4] Notably, Skt. t, th, d, dh, n, s became ṭ, ṭh, ḍ, ḍh, ṇ, ṣ by the influence of preceding sounds, some of which were consonants and some were vowels.

[5] But cf. the preceding footnote.

[6] But conversely, in German the ch is palatal or velar according to the quality of the preceding vowel; this is natural when the ch is final in the word or in the syllable, as in (velar) Bach.   When we find velar ch in Rache and palatal ch in rächen, the fact that the preceding vowel governs the quality in Rache indicates a certain semantic unity with the preceding, a feeling that the root is rach-, despite the syllabic division Ra-che.

[7] The classical treatise on this subject is Maurice Grammont, La Dissimilation Consonantique dans les langues indo-européennes et dans les langues romanes (Dijon, 1895).   Grammont goes too far in setting up formulations which operate almost as regular phonetic laws; rather we should limit 'regular' dissimilation to a few formulations in specified languages.   His examples are virtually all of regressive character, though he professes that his 'laws' I–XVI are indifferently regres-

When two like sounds occur in proximity, there is often, as Carnoy has shown[8], a tendency to suppress or alter one of the identical muscular movements in their articulation; the suppressed or altered movement is regularly in the prior sound, which is thereby changed.   For example: Greek ἀργαλέος 'painful', from *ἀλγαλέος, cf. ἄλγος 'pain'; Oscan **diumpā-**, from Greek νύμφη 'nymph'[9]; Latin *caerulus* 'sky-blue', from *kailolos*, cf. *caelum* 'sky'; Italian *albero* 'tree', from *arbero*, from Latin (acc.) *arborem*; Latin *meridiēs* 'noon', from loc. *med(i)ī diē*.[10]   Perhaps the most general application of this principle is seen in the formulation known as Grassmann's Law: that in Prim. Indo-Iranian and Prim. Greek, when two aspirates occur separated by one or two vowels, the prior aspiration disappears.   Thus the root *dhē* appears reduplicated in Skt. *dádhāti* 'he puts', and in Greek τίθησι; further Greek θρίξ 'hair', gen. τριχός; ἔχω 'I have', fut. ἕξω.

But here we find in Greek a conflict with another principle: that when dissimilation brings about a change rendering more difficult the identification of the form as belonging to others of its etymological and semantic kin, the direction of dissimilation may be altered so as not to obscure the meaning of the word-form.   There is an ending *-dhi* which appears in some forms of the second person singular imperative, as in Skt. *edhí* 'be thou', Greek ἴσθι, from *es-dhi*; Skt. *ihí* 'go thou', Greek ἴθι.   This ending is applied to forms of the second aorist passive in Greek, which have a suffix -η-: thus φάνηθι 'appear thou'.   But the tense suffix of the first aorist passive is -θη-, as in ἐλύθη 'he was loosed'; the second singular imperative should be *λύτηθι from -θηθι, but it is actually λύθητι.   For the tense-sign is more important for the hearer's understanding, than is

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sive or progressive.   An examination of the progressive dissimilations which he cites shows that in virtually all there is some special factor to which the reversal of the natural direction of the influence may be attributed.   Thus Eng. *marble*, from French *marbre*, may have been influenced by the numerous words ending in *-ble* in English; Ital. *propio*, from Latin *proprius*, lost the second *r* rather than the first by the influence of the many words which begin with *pro-*; Ital. *frate*, from Latin *fratrem*, probably lost the second *r* rather than the first, by the influence of *fratello*; etc., etc.   Some of these influences are indeed suggested by Grammont, though he fails to see their significance.

[8] The Real Nature of Dissimilation; TAPA 49.101–13; though this explanation, in less precise form perhaps, was current long before Carnoy's article, and was formulated by Grammont (16) in his first two principles.

[9] J. Wackernagel, Archiv f. lat. Lex. 15.218–21.

[10] Or the sound may be entirely lost, as in Span. *postrado* 'prostrate, humbled', from Latin *prostratus*.

the personal ending; therefore the personal ending appears in a revised form, with a reversal of the direction of dissimilation.

Here we have met a very important factor: the semantic value of the sound as part of a word; and we are justified, as we shall see, in setting up a formulation of a psychological nature, that when the normal phonetic change disturbs the ready understanding of the meaning, then a compensation of some character will enter in, and the expected phonetic change may be replaced by another change which does not so interfere with the comprehension. I call this the semantic value of the sound or sounds: not the symbolic value, which refers to the value of sounds at the very time when the words were acquiring a conventional meaning, a value which they are sometimes alleged to have before the actual words ever acquired a conventional meaning.[11] The semantic value of the sound is that which it has in its conventional environment, an environment which will be disguised by too violent a change of an important phonemic factor. Of this I have already given you one example, in connection with Grassmann's Law in Greek; I shall now go on to other phenomena of the same category.

In a cluster of three consonants, the first and third homorganic stops and the middle sound a sibilant, it is regularly the prior of the homorganic stops which is lost. Thus *ksk* becomes *sk*, *psp* becomes *sp*, *gzgh* becomes *zgh*, etc. Familiar examples are Greek διδάσκω 'I teach', from **didakskō*; Latin *discō* 'I learn', from **di-dk-skō*; Greek βλάσφημος 'evil-speaking', from **-p-s-phāmos*; Latin *aspellō*, *asportō*, from **aps-p-*, cf. *abs-cīdō*, *abs-tineō*, where the *b* (graphic for [p]) has remained by analogical influence; Greek ἕξ 'six' in compounds and ἐξ 'from' in phrases should lose the *k* before guttural stops, and the *s* before other stops: the loss of *k* is seen in Boeot. ἐσκηδεκάτη 'sixteenth', Cret. ἐσκλησία 'meeting', Thess. Boeot. Cret. ἔσγονος 'descendant', and this value is generalized in these dialects and in Arcadian: Thess. ἐσδόμεν, Boeot. ἐς τῶν, etc. But in other dialects the values ἐκ- and ἐκ- (regular before consonants other than κ γ χ) were generalized, as in Attic ἐκκαίδεκα, ἐκκλησία, ἐκ τῶν, etc.

The same factor operates in reduplications of roots beginning with *s* + stop: root **st(h)ā-*, Skt. present *tiṣṭhati*, perfect *tasthāú*, Avestan pres. *hištaiti* (*h-* from *s-*), Latin *sistō*, Greek ἵστημι, with variation as to which consonant is lost. But here Latin shows a special influence in the perfect tenses: *spopondī*, *scicidī* (older form for later *scidī*), *stetī* and

[11] Cf. the discussion by Otto Jespersen, Language 396–411 (chapter on Sound Symbolism; New York, Henry Holt, 1922).

stitī, from *spondeō*, *scindō*, *stō* and *sistō*, show loss of *s* in the second cluster, probably because of association with the present form, in which the cluster begins the word.[12]

Certain Latin suffixes of frequent occurrence also show the effects of dissimilation: notably *-ālis* and *-klom* (from *-tlom*). When *-ālis* stands at the end of a word in which there is an *l*, then dissimilation takes place; because the main part of the stem is more important for the understanding of the word than is the suffix, the normal course of dissimilation is reversed, and the dissimilation is progressive, resulting in a new suffix *-āris*. Examples, from the older language: *annālis, capitālis, līberālis, mortālis*; but with *-āris, familiāris, mīlitāris, populāris*.[13] Of the normal regressive assimilation there may be a trace in *parma* 'small round shield of a cavalryman', if it is a back-formation from **parmālis* for **palmālis* 'held in the hand', from *palma*.[14] There was also the adj. *palmāris*, with progressive dissimilation; but *palmāris* retained its semantic association with *palma*, while *parma* diverged widely in this respect, and so was not under its influence.

The same is seen in the suffix *-klo-* (from *-tlo-*), which appears in *vehiculum, perīculum, pōculum*, but with dissimilation in *lucrum, sepulcrum, simulācrum*. Similarly in the suffix *-dhlom*, fem. *-dhlā*: *pābulum, stabulum, vocābulum, fābula*; but also *flābrum, candēlābrum, palpebrae*. In all these, and possibly some others, the normal direction of dissimilation is altered because it is easier to recognize two suffixes than to understand two forms of the fundamental stem in each and every set of words. There is an obvious psychological economy.

The rhotacism of Latin, the change of original intervocalic *s* to *r*, also becomes involved with dissimilation.[15] Thus while *s* became *r* in gen. *generis, honōris*, in *cūra, nurus*, etc., the *s* remained in *miser* and *caesariēs* because of the retarding influence of the following *r*. In *dirimō* and *diribeō* the *s* of the prefix was rhotacized; but in *dēsiliō, dēsinō*, and the like, the *s*, initial in the second element, remained unchanged because of its importance for the understanding of the words. We say that the *s* is retained by analogy; but we must contemplate the difference be-

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[12] It is to be noted especially that the apportionment of *stetī* to *stō* and of *stitī* to *sistō*, is not original, but is a relatively late development.

[13] Cf. my Sounds of Latin §176.II. There are nearly 200 Latin words in *-āris*, with a preceding *l* somewhere in the word; other words in *-āris* are late remodelings of words in *-ārius*, or are from Greek.

[14] M. Niedermann, Essais d'Étymologie et de Critique verbale latines 36–45, in Recueil de travaux No. 7, Univ. de Neuchâtel (1918).

[15] Cf. my Sounds of Latin §166.II.

tween the development in *dirimō* and that in *dēsinō*, and must seek to interpret it.   Note also that *dēsīderō* keeps its *s* although there is no uncompounded **sīderō*; *sīdus*, to which it is a denominative verb, is far away semantically; any analogical influence which there is, was exerted by *cōnsīderō*, also rather far away in meaning—and I look upon the fact of the initial position in the second element of the compound as the strongest factor in the case—here again, a psychological moment.

In the primitive IE, the combination of dental stop + dental stop yielded an affricate group, eventuating in *tst(h)*, *dzd(h)*, which had various fortunes in later languages.   If we may take *tst* as type, because of its relative frequency, we find three products in later times: *tt* as in Sanskrit, *st* as in Avestan, *ss* as in Italic.   I have elsewhere explained the development to *ss* in Italic in the following way:[16]

These dental clusters are in practically all instances the product of attaching a suffix beginning with a dental, to a root ending in a dental. Such a cluster would normally lose the prior of the homorganic stops by dissimilation; as in Avestan *st*.   But this tends to obscure the relation of the derivative to the original root; and as with the Latin suffixes *-ālis* *-āris*, *-culum* *-crum*, *-bulum* *-brum*, the suffix rather than the root suffered the change.   So from *mittō* the participle **mitstos* became not **mistos*, but **mitsos*, showing its connection with the root *mit-*; that this cluster *ts* later became *ss* in *missus* 'sent', does not concern the earlier history.   But in this way a peculiar double set of endings, *-tos -sos*, *-tiō -siō*, *-tor -sor*, etc., came into being, precisely as did the double forms of the other endings which have been mentioned.

Inasmuch as these clusters were almost invariably the product of derivation, there was in every such derivative the possibility of analogical influence.   I am inclined to look upon the Sanskrit *tt* also as a similar product of the psychological urge to retain the identity, in this instance both with the root and with the suffix.   Thus when **setstos* became *sattás*, it showed its relation to the root *sad* and to the suffix *-tas*; when **yoitstha* became *vettha* 'thou knowest' (= Greek οἶσθα), it kept its apparent (and real) relation to *veda* 'I know', as well as the ending *-tha* which is seen in other verbs.

The same element appears in the development of such a cluster as *dzdh* from *dh* + *t*, as in the participles of roots ending in the voiced aspirate.   The product might have been *tth* with the unvoicing of the prior stop in the cluster, and the metathesis of the aspiration—then the development to *tsth*.   But instead, contrary to all other parallel changes,

[16] LANG. 8.18–26; also in Sounds of Latin §144.

the entire cluster became voiced, by the progressive influence of the voiced aspirate.   I take this to indicate that the voiced quality of the final consonant of the root seemed to be important for the appreciation of the meaning: that is, that the progression of the voiced quality was not a mechanical phonetic process, but a psychological one.   Thus *bhudh-tos became *bhudzdhos, then with Grassmann's Law buddhas 'awakened', in Skt., wherein bud- shows the relation to the root in its Skt. form budh-, far better than if *bhudh-tos had become *bhutsthos, yielding a Skt. *bhutthas.   As a result, Indo-Iranian (which shows the products clearly) has two participial endings: -ta- (after non-dentals) and -dha- (after dental stops) in Skt., -ta- and -da- in Iranian (where analogies have eliminated many examples of -da-).   This is very similar to the status of the Latin suffixes.   The development of -dzdh- to Skt. -ddh- I take to be similar to that of -tst-.   A few examples which seem to indicate a true phonetic development, with -st- and -zd(h)- (whence further developments), are cited in my previous article.[17]

To return now to phenomena of assimilation, the same importance, which I have called semantic importance, of the sounds, appears in the word prope.   The superlative proximus betrays an original *proque: now in such a combination of sounds, an initial p and a medial kw in the same word, there was in primitive Italic (and in primitive Celtic) an assimilation of the initial p to the medial kw—a regressive influence being exerted.   This would have led to a positive *kwrokwe, becoming *croque; but the superlative proximus would be exempt from this influence, because the -w- of the kw would be lost before the consonant of the suffix.   A series *croque, comp. *croquior or *procior, superl. proximus, would be too irregular for utility; and the direction of the assimilation was reversed, giving prope, propior, proximus, forms supported by the familiar adverb (preposition and prefix) pro.

Possibly now a few examples from recent spoken English, will demonstrate the direction of assimilation even better than those drawn from older languages.   When John D. Rockefeller Jr. reversed his attitude toward the Prohibition Amendment, he read an address which was recorded for the moving pictures; in it he said 'the abolution-lition of the saloon.'   Clearly the approach of the -lū- in saloon caused him to replace the -lĭ- of abolition by -lū-.   Again, when Lowell Thomas, on June 23, 1936, was broadcasting the news of the day, he said, 'When the Democratic confession—convention was in session. ... '   It is obvious

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[17] LANG. 8.25.

that the coming of the word *session* affected his first articulation of
convention.   In both instances the speaker corrected himself at once;
but these examples, and others which I have noticed from time to time,
without recording them, illustrate my remark, made much earlier in
this address, that the thinking of the speaker is ahead of his utterance,
and the attempt to catch up with the thought results in regressive assimi-
lations.   This, I take it, is the true nature of assimilations; and simi-
larly the true nature of dissimilations is an avoidance of identity with
what is coming.

There are examples of such progressive assimilations of a psychological
origin, in consonantal clusters developing in phrases and compound
words.   For example, the pronunciation of *Miss de Greene* and that of
Mr. Greene, in rapid articulation, are virtually identical; for a cluster *sd*
must become either *zd* or *st* in rapid speech, and the *s* of *Miss* is semanti-
cally more important than the *d* of *de Greene*.   We do not readily under-
stand *Miss* if it is pronounced [mɪz]; the voiceless *s* must be kept.

Similarly, we do not easily recognize the prefix *dis-* when it is sounded
[dɪz].   The word *disaster* is nearly always pronounced with [dɪs], instead
of the standard [dɪz].   *Disperse* and *disburse* are confused in speaking
and in writing: cf. 'his widely disbursed engineer troops' (in The New
York Times for July 12, 1931), and 'dispersed funds' (seen by me, but
source not recorded).   Then also *disdain* becomes *distain* (which is
quite a different word), as in 'distain for his own world' (review by
Margaret Wallace, in The New York Times Book Review for Oct. 8,
1933, page 6).   In strictness, the pronunciation of these clusters be-
comes not precisely *-st-* and *-sp-*, but rather *-std-* and *-spb-*, with a shift
to the voiced quality in the middle of the stop; but this phonetic feature
is outside the normal pattern of English, and is not heard as such by the
listener, any more than he hears the aspiration of initial voiceless stops;
neither feature is phonemic in English.

The Old High German consonant variation known as Notkers
Anlautsgesetz,[18] is also in part to be included here.   This law is that
OHG initial *b*, *d*, *g* became voiceless initial in a sentence or clause, and
medial in a clause if the preceding word ended in a voiceless consonant;
also when initial in the second element of a compound word, the prior
part of which ended in a voiceless consonant.   In view of the unvoicing
of final stops, the sounds which for this formulation count as voiceless

[18] Cf., for example, Wilhelm Braune, Althochdeutsche Grammatik[2] §103
(1891); V. Michels, Mittelhochdeutsches Elementarbuch[3] §178 (1921); etc.

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are those represented by final *p, t, k, b, d, g, f, h, z, s*.   Examples: *ter brûoder* (= *der Bruder*, initial in sentence or clause); genitive *des prûoder* (medial in sentence or clause); *únde daz, íst taz*; etc.   In compounds, for example: *hímilbûwo*, but *érdpûwo*.   We must divide the phenomena into two classes: initial in the sentence or clause, the voiceless sounds are really 'voiceless fortes',[19] as in certain Modern German dialects (Saxon, Northeastern Bavarian, etc.), with which we are not concerned; within the clause, after voiceless sounds, the initial sounds suffer progressive assimilation as to voice.   For in this instance the finals have more semantic significance than the initials: some are indicative of cases, like the *-s* of the genitive and of the neuter, and others have not so long before this been changed from voiced finals to voiceless, in the process carrying with them the initials of the immediately following words.

This unvoicing of the initial did not become standard in any considerable number of words, if indeed in any words, without some special provocation.   There is at least one word in which we can identify the special reason.   German *tauen* corresponds to English *thaw*; by the normal correspondence of sounds we should expect **dauen*.   There was actually an OHG *douen*, but the MHG is *touwen*; the original word survives in NHG *verdauen*.   The explanation is that this word is common in but two phrases, *es taut* and *das Eis taut*, in both of which the preceding sound is a voiceless *s* which is not subject to variation in sentence-sandhi, without obscuring the meaning.[20]   Accordingly the assimilation is of a progressive nature.

Another word which must be discussed in this light is German *tausend*.   All the other Germanic dialects agree with English *thousand* in going back with regularity to an initial voiceless stop, which should appear in OHG as *d*.   In fact the OHG word was *dūsunt* or *thūsunt*; but in late OHG times the spelling *tūsent* came into use, indicating a change to a voiceless initial.[21]   MHG had regular *tūsent*, sometimes *tūsunt*; thus the change was a real change, not merely a local dialectal

[19] And the *b, d, g* which correspond, were 'voiceless lenes'.   The formulation given above, however, is not misleading, since the processes are as there stated.

[20] Calvin Thomas, Practical German Grammar⁴, App. II, §8.3.a (p. 411).   E. C. Roedder, who called my attention to this passage, states that Thomas took this explanation from a paper read by Georg Hempl; but I cannot locate the latter.

[21] This explanation was given briefly by C. Thomas, l.c., and by Hermann Paul, Deutsche Grammatik I. §200 (1916).   A few other examples of the same shift, probably due to the same cause, are listed by Paul, op. cit. §202.   The various forms of *tausend* in the older documents, are listed by J. Schatz, Althochdeutsche Grammatik §192, §193, §410, §414 (1927).

change, or a temporary variation in orthography.[22]   In the absence of a word beginning with *t* that might have exerted an influence because of semantic association—similarity or antithesis of meaning—we must take recourse to sentence-sandhi as the probable cause of the change. Notably, a word meaning '1000' is likely to be used with a preceding numeral multiplier:[23] the units 1 to 10, the tens 20 to 90, and 100 are the words which will commonly precede '1000', more than will any other delimitable set of words.

Now a number of these numerals ended in voiceless stops or spirants. There are *hunt* '100', which was not replaced by MHG *hundert* until the twelfth century; the multiples of '10',[24] ending in *-zug* or *-zuc* (with variant spellings), becoming MHG *-zic* or *-zec*, now NHG *-zig* (with *-g* graphic by analogy of derivative forms where a vowel follows); *fimf*, *finf*, MHG *fümf*, *finf*, NHG *fünf*, and *sëhs*, MHG *sehs*, NHG *sechs*.[25] Even *einlif* '11' and *zwelif* '12' may be counted here.   The variation of the initial of '1000' would then, under the principle of Notker's Law, have been very active; and the form *tūsent* would have further support in the ending of the ordinal numerals: *ēristo* and *furisto* 'first', *drittio* and *dritto* 'third', *fimfto* and *finfto*, *sëhsto*, *sibunto*, *niunto*, *zëhanto*.[26] Seven of the first ten ordinals had the suffix containing *t*, which was destined to spread to the remaining three.   Thus there was a natural model given by the ordinals, for a cardinal with a following *t*: *fimfto* and *fimf tūsent*, *sëhsto* and *sëhs tūsend*, seem to form natural complementary pairs.

There is another well-known instance of such a progressive assimilation in German, in sentence-sandhi.   The second person singular of the present tense of the verb ended in *-s*, to which the subject pronoun *du* (also written *thu*, but indicating the same pronunciation) was added in

[22] On dialectal variations, cf., for example, H. Paul, op. cit. §200; L. Sütterlin, Neuhochdeutsche Grammatik 242 (1924); O. Behaghel, Geschichte der deutschen Sprache⁵ §396 (1928).

[23] It would be unprofitable to list examples from the literature; such combinations would be more frequent in actual speech than in literary compositions.

[24] OHG *zëhanzug* '100', with variants in *-zeg* and *-zech*, may be understood as included with the multiples of '10'.

[25] At that time '8' was still *ahto*, with the final vowel.

[26] W. Braune, Ahd. Gr.² §278, cites OHG *sibunto*, *niunto*, *zëhanto*, with *-to*; H. Paul, Deut. Gram. I. §202, quotes MHG *siebende*, *neunde*, *zehnde*, with the voiced dental, coming down into the 18th century, when the *d* was finally ousted by the *t*. The probable variation in OHG times can hardly be taken as invalidating the argument which I give above.

the inverted sentence-order; the result was that the voiced initial became voiceless when in contact with the voiceless final. For the reverse change, to the voiced value, was contrary to the speech-habits of the Germans in uttering final consonants; the -*s* was significant of the form. Thus we find such writings as *lisistu* 'liesest Du', *thenkistu* 'denkst Du', etc. From such combinations came the regular modern German personal ending -*st*.[27]

A similar combination in English also produced the ending -*st* of the second person singular of verbs; the elements were here a final *s* and an initial dental spirant (not a dental stop).

Another possible, but not certain, example of progressive assimilation is seen in Spenser's *note* = *ne mote*, Faerie Queene V.3.7. *Note* normally stands for *ne wote* 'knows not', OE *nāt* from *ne wāt*; but the meaning in the passage cited seems to be 'could not':

> cruell fight ...
> In which all strove with perill to winne fame;
> Yet whether side was victor note be ghest.

If this be a correct view,[28] then for metrical reasons Spenser shortened *ne mote* to *note*, in imitation of the regular *note* for *ne wote*; while an *n* would regularly be assimilated to a following *m*, in a combination like *nmote*, this was here impossible, since the negative form would be identical with the positive.

The principle of a semantic value of sounds serves also to explain two forms found in the Oscan inscriptions, which have as yet received no satisfactory interpretation: **ekkum** 'likewise' and **fússu** 'the same' (nom. pl. masc.). It has been recognized that **ekkum** (found twice on the Cippus Abellanus,[29] lines 27 and 41) is an adverb from the stem **eko-**, corresponding in meaning to the Latin *hic* 'this', with the addition of a particle -*dom*, indicating identity, like Latin -*dem*. But from **ek-dom*, wherein a medial vowel has been lost by syncope, it has seemed impossible to get **ekkum**. The explanation lies in a progressive assimilation

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[27] Cf. W. Braune, Ahd. Gram.[2] §306 Anm. 5.

[28] This is the interpretation of H. W. Sugden, The Grammar of Spenser's Faerie Queene §288; cf. G. O. Curme, Grammar of the English Language III, §49.4.c.(1).a, and Hereward T. Price, A History of Ablaut in the Strong Verbs from Caxton to the End of the Elizabethan Period 30 (Bonner Studien zur Englischen Philologie, Heft 3). As to another view, it seems to me hardly possible that *note* is here for *ne wote*, with the acquired meaning of 'could not' (from 'does not know how'), as in French *je ne saurais* 'I should not know how to', hence 'I cannot'.

[29] R. S. Conway, Italic Dialects No. 95; R. von Planta, Gram. d. osk.-umb. Dial. No. 127; C. D. Buck, Oscan-Umbrian Grammar No. 1.

in a consonant group which was otherwise not present in the language: the psychological need of keeping a relation to the stem *eko-* caused the second consonant to be assimilated to the first, instead of some change in the prior sound with retention of the second.

The same phenomenon is seen in **íússu**, on the road-builders' tablet of Pompeii.[30]   The nominative ***íús**, from **eōs* 'ii', plus the enclitic *-dom*, the particle of identity, yielded ***íúsdum**.   The combination *sd* did not occur unchanged in Oscan, and to avoid obscurity through separation from the form ***íús**, a progressive assimilation took place, yielding **íússu**, in which the final *m* is not written.   Cf. with this the Latin genitive *eiusdem*, retaining *-sd-*, a cluster in which otherwise the *s* is lost with lengthening of the preceding vowel: the *s* is kept for clarity, although in the nom. sg. masc. *īdem*, from **is-dem*, the *s* has been lost. But *īdem* was a much more common form, and could stand on its own feet, without support from another form.   As for the dat.-abl. *īsdem* and the acc. pl. *eōsdem* and *eāsdem*, the retention of the *s* was essential to prevent them from becoming identical with the nom. pl. masc. and the abl. sing. respectively.   The difference between Latin and Oscan was that Latin tolerated the exceptional *-sd-* when it was useful, but Oscan got rid of it by a progressive assimilation.

There is a slightly different situation in two types of progressive assimilations in English, though not at variance with the principle of semantic importance.   In the past tenses of the type *baked*, after the loss of the second vowel, the final cluster *-kd* became [kt].   This was essential for preservation of the relation to the present *bake*; had there been regressive assimilation of the voiceless *k* to the voiced quality of the *d*, then there would be confusion with *begged*.   In most words there would be no such resultant homophony[31]; but in any instance, at the

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[30] Conway No. 39, von Planta No. 28, Buck No. 3.   The first occurrence is clearly written, except that the second letter may be **u**.   The second occurrence has lost the first **s** and most of the preceding letter, at the end of the line, where moreover there is space for but two letters of the word; the **su** on the next line is clear.

[31] Languages differ widely in their tolerance of homophones; Greek, Latin, German have relatively few, French, English, Chinese have many.   Yet Chinese must often relieve the situation by adding a class-word or a synonym to the ambiguous word; occasionally English employs a similar device.   On the avoidance of homophones, cf. A. Meillet, Sur les Effets de l'Homonymie dans les anciennes Langues indo-européennes, in Bibl. de l'École des Hautes-Études 230.1C9–80 (1921); J. Gilliéron, Les Conséquences d'une Collision lexicale et la Latinisation des Mots français, ib. 54–74; and my article "No Tresspass" in Latin Linguistics, in Classical Studies in Honor of John C. Rolfe 143–61 (1931).

end of the word the decreasing energy of articulation would tend to make the final sound yield to the influence of the preceding. This factor of decreasing energy is clearer in the second type, namely the (substandard) pronunciation of *post* as *pos*, *field* as *fiel*, etc., with a lengthened final consonant: but we should never recognize these words if they had undergone regressive assimilation in the final cluster, giving *pot* and *fied*—the prior phoneme is here the characteristic of the cluster, and must survive.

An examination of many examples of assimilation and dissimilation of consonants shows that the natural direction of the influence is regressive; I have attributed this to the fact that the thought of the speaker is ahead of his utterance, which tries to overtake the thought, but only at the expense of confusion in the order or the nature of the sounds uttered. To a considerable extent the same is true of vowels as well, though not to quite the same extent. But it seems to me clear that when the direction of assimilation or of dissimilation is progressive, there should be made a special study of the example or set of examples, to determine the special reasons for the extension of the influence in the direction contrary to that which is normal to human speaking. In most instances this is found to be in the semantic value which attaches to the sounds, in that the change of the prior of the two sounds will obscure the connection of the word with others of the same etymological and semantic group. This is, as I have said, not an automatic physiological factor, by which one muscular movement in the articulation is omitted or altered, but a distinctly psychological factor.

TIBETAN INFLUENCES ON TOCHARIAN, I

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As far back as 1922, in an article[1] which in many respects remains the most significant contribution yet made to the larger understanding of the status of Tocharian, Eduard Hermann pointed out the importance of Tibetan because of its far-reaching morphological influence on Tocharian. Little attention, however, seems to have been paid to this aspect of the Tocharian problem, yet it is safe to say that one can not take a step in interpreting the forms of Tocharian, syntax and morphology both, unless one bears in mind the moulding influence exerted on Tocharian, both A and B, by this non-Indo-European language. In brief, Tocharian is a Tibetanized Indo-European idiom. Once this is understood, the much discussed problem of whether both A and B were spoken in Chinese Turkestan or B ('Kuchean') alone, the A manuscripts appearing in the eastern part of the area merely because they were brought there by speakers of B from a real or supposed western home in Tokharestan (Bactria), falls by the wayside as an utterly pointless question. If dialect A has specifically Tibetan features in its structure—and it has—how could these have developed except through actual contact between speakers of Tocharian A and speakers of Tibetan, and where could such contact be had except in Chinese Turkestan itself, at a point not far removed from linguistically Tibetan territory?

In the present paper we shall take up a number of specific points which seem to argue for a measure of Tibetan influence on Tocharian.

1. TOCHARIAN 'HEART-FATHER'

In §373a the authors of the Tocharische Grammatik[2] give us an example of a tatpuruṣa compound with substantive as first element: āriñc-pācar 'heart-father'. The form occurs in the phrase kāpñe āriñc-pācar 'dear heart-father' in two passages of the Tocharische

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[1] A review of Sieg und Siegling, Tocharische Sprachreste, I. Band. Die Texte (KZ 50. 296–314 [1922]; see 309–11).

[2] Referred to as SSS, i.e., E. Sieg, W. Siegling, and W. Schulze.

Sprachreste (356 b 3; 407 a 3).   This curious turn of phrase can also be
expressed in adjectival form, i.e., by means of an adjectival derivative
of *āriñc* in *-i* or *-și* (SSS §§42; 44a).   As it happens, these adjectives
occur with 'son' and 'sister', not with 'father', but the total number of
examples of both types of usage is not large enough to justify the infer-
ence that there is a specific difference of idom in the use of 'heart' with
'father' (and 'mother') on the one hand and with 'child' and 'brother'
or 'sister' on the other.   It is far more likely that one might say either
āriñc-pācar or **āriñci pācar*, **āriñcși pācar*.[3]   The adjectival examples
are:

> *āriñciṃ se-* (338 b 7: 'heart son'.   *se-* broken off but presumably to be
> restored to instrumental *seyo*, requiring oblique m. form in *-ṃ* of
> preceding adjective, and pairing with preceding *märkampalșiṃ*
> *tuṅkyo*)
>
> *āriñcșināṃ se* (Frgm.: 'heart son'; obl. m. sing.)
>
> > *kāpñe āriñcșinäs sewās* (356 b 1: 'dear heart sons'; obl. m. plur.)
>
> *āriñcșināṃ șar* (451 a 4: 'heart sister'; obl. f. sing.)

Parallel to such a Tocharian form as *āriñcșinäs sewās* 'heart sons',
which could presumably alternate with compounded **āriñc-sewās*
(ob. m. plur.) 'heart-sons', would seem to be Tibetan *t'ugs-kyi sras*
'heart's son' or *t'ugs-sras* 'heart-son'.[4]   The actual meaning of the term,
according to Jäschke, is 'spiritual son, an appellation given to the most
distinguished scholars or saints'.   *Sras* is the 'respectful' word for 'son'
in Tibetan, *bu* the ordinary word.   Similarly, *t'ugs* is the respectful
word corresponding to *sñiṅ* 'heart, breast, mind', *yid* 'soul, mind', *sems*
'soul', and other words of mental or psychological connotation.[5]   The
Tibetan *t'ugs-kyi sras*, a genitive construction, seems to correspond to
the adjectival Tocharian *āriñcși se*, the Tibetan *t'ugs-sras*, a compound,
to a Tocharian compounded **āriñc-se*.

The use of Tocharian *āriñcși* 'heart' (adj.) in other cases seems to
correspond rather closely to that of Tibetan *t'ugs-* in comparable com-

[3] Inasmuch as *c* represents a palatal affricative, roughly *tś*, the two forms
āriñci and *āriñcși* are likely to be merely orthographic variants of a single form in
-și, the most common Tocharian adjectival suffix.   If this is correct, *āriñci* (occur-
ring only as an oblique sing., *āriñciṃ*) would not be formally parallel to such adjec-
tives in palatalizing *-i* as *ñäkci* : *ñkät* 'god' and *mañi* : *mañ* 'moon, month', as is
assumed by SSS.

[4] See Jäschke, A Tibetan-English Dictionary, s.v. *t'ugs* (233).

[5] For 'respectful' terms in Tibetan see Jäschke, Tibetan Grammar, Addenda by
A. H. Francke assisted by W. Simon 35–6, 131–6 (1929).

pounds.   Thus, *āriñcṣi ākāl* 'heart wish' (nom. m. sing.)[6] parallels such
Tibetan compounds as *t'ugs-dam*[7] (the respectful analogue of *yi(d)-dam*)
'a prayer, a wish in the form of a prayer' (= *smon-lam* 'wish-road'; cf.
yid-smon 'soul-wish, wish') and *t'ugs-dgoṅs* (= *dgoṅs-pa* 'wish') 'heart-
wish, will'.   Another such Tocharian collocation is *āriñcṣinäṃ yärṣlune*
'heart homage' (obl. m. sing.),[8] for which I have not found an exact
Tibetan parallel.

All these Tocharian-Tibetan parallels may be merely due to literary
transcriptions, in which case one may be tempted to argue back to
Tibetan originals for at least certain of the Tocharian A texts.   It is
difficult, however, to see nothing but a slavish imitation of a Tibetan
model in such a compound as *āriñc-pācar*, an intimate term, the more
so as there seems to be no specific parallel, say **t'ugs-p'a*, for this
Tocharian word.   (The respectful form for Tibetan *p'a* 'father' is
another word, *yab*).   The striking resemblances in vocabulary between
Tocharian and Tibetan involving the word 'heart' (particularly *āriñcṣi
se* 'heart son' : Tib. *t'ugs-kyi sras* or *t'ugs-sras* and *āriñcṣi ākāl* 'heart
wish' : Tib. *t'ugs-dgoṅs*) coupled with stylistic resemblance even where
there is difference of detail (e.g., *āriñc-pācar* 'heart-father', presumably
modeled on such cases of Tocharian *āriñc-* or *āriñcṣi* as do correspond to
Tibetan examples of *t'ugs-* or *t'ugs-kyi*) indicate rather that we are
dealing with a general influence on Tocharian word formation, which
could act creatively within Tocharian itself and which is therefore prob-
ably based on actual folk usage.

2. TOCHARIAN 'PITY'

The Tocharian term for 'pity', corresponding to Sanskrit *karuṇā*, is
kāryā lotklune, in which *kāryā* can hardly be other than the old stem of
kri, kāry-, translated by SSS as 'will'.[9]   The word *lotklune* is a verbal
substantive, 'turning', belonging to the verb *lotk-* 'to turn (to), to turn
about, to become'.[10]   Hence Tocharian 'pity' may be rendered 'a turn-

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[6] SSS §44a; Tocharische Sprachreste 58b 2.

[7] Jäschke, A Tibetan-English Dictionary 233.

[8] Tocharische Sprachreste 6 b 2.

[9] SSS §388 b.   The occurrences in Tocharische Sprachreste are: 426 b 2 (*kāryā
lot(klu)ne*, apparently translating Sanskrit *k[aruṇa]yā*); 399 a 5 (*tām k_uleyaṃ
kāryā lotklune* 'pity for that woman'); 399 b 1 (*wākmats kāryā l..ku-*, broken off,
apparently 'a special compassion', with *l(ot)ku(ne)* miswritten for *lotklune*?);
and 465 a 2 (*kāryā lot[klu]-*, broken off).

[10] SSS 467.

ing of one's will (toward one)'.   This is only a first approximation to the
precise meaning, as we shall see in a moment.   The words *kāryā lotklune*,
though written separately, should be thought of as a compound noun,
'will-turning', *kāryā-* being the stem form of *kri* (nom. sing.) very much
as *wsā-* 'gold' (e.g., in such compounds as *wsā-yok* 'gold-colored' and in
adjectival *wsāṣi* 'golden') is the old stem form of *wäs* 'gold'.[11]   This
seems to be indicated by the ablative form *kāryāṣ* (in *puk āñmaṣ kāryāṣ*
'from [one's] whole soul [and] will'), not **kāryäṣ* (which would be ex-
pected if *käry-* were the true stem),[12] and by the adjectival derivative
käryatsum 'intending', which is better analyzed as *kärya-tsum* with *-ā-*
shortened to *-a-* (such secondary *ā : a* ablaut is common in Tocharian)
than, more mechanically, as *käry-atsum*.[13]

The Tibetan terms for 'pity, compassion' are *t'ugs-rje* (respectful),
sñiṅ-rje, *t'ugs-brtse-ba* (respectful), and *sñiṅ-brtse-ba*.   The *t'ugs-* and
sñiṅ- of these compounds are the respectful and normal forms for 'heart,
mind' with which we are already familiar.   *brtse-ba* is the verb 'to love'
and its corresponding substantive 'love, affection, kindness'.[14]   Hence
t'ugs-brtse-ba and *sñiṅ-brtse-ba* merely intensify the meaning already
given by the simplex by classifying it under the generic head of psycho-
logical experience ('heart'), a type of formative process which is very
familiar in all Sinitic languages and which probably has reflexes in
Tocharian.[15]

[11] SSS §363 c.
[12] SSS §208.   *-āṣ* is here referred to as 'die vollere Endung', while *kāryā* above
is interpreted as the *ā*-case of *kri* (§200 b) petrified into an adverb (§388 b).
[13] SSS §37.
[14] Jäschke, A Tibetan-English Dictionary 442.
[15] E.g., under the category 'heart' comes 'thought': *āriñc pältsäk* 'heart thought';
under the category 'pain' come 'sorrow' and 'doubt': *klop śurām* 'pain sorrow',
klop sañce 'pain doubt'; under the category 'love' come 'belovedness', 'homage',
'worship', 'friendliness': *tuṅk kāpñune* 'love belovedness', *tuṅk poto* 'love hom-
age', *tuṅk ynāñmune* 'love worship', *tuṅk ylārone* 'love friendliness'.   Examples of
this sort are not recognized by SSS as involving notions of classification but are
interpreted, reasonably enough, as illustrating dvandva-like compounding,
frequently with synonymous meaning of members, e.g. 'love (and) homage', cf.
śla tuṅk poto yo 'together with [= one provided with] love and homage'.   See
SSS §358.   They point out, however (§359), that such compounds are often treated
as singulars, e.g., *klop sañce wikāluneyaṃ kälkāṃ* 'pain doubt in-disappearance
went (sing.)-for-her', which they translate as 'Schmerz (und) Zweifel verging
[= vergingen] ihr' but which, on the analogy of such Tibetan forms as *sñiṅ-
brtse-ba*, one is tempted to understand rather as '(that) pain (which is) doubt
disappeared from her'.   Naturally, there can be no sharp line of division between
the 'classifying' compound and the synonym compound.

As for the *-rje-ba* (*-ba* is a substantivizing article making abstract nouns, adjectives, and 'infinitives', to adopt current terminology) of *t'ugs-rje-ba* and *sñin-rje-ba*, it is clearly not identifiable with the *rje* (*rje-ba*) that means 'lord, master', but with the verb *rje-ba* that means 'to barter, to give or take in exchange' or, in a more general sense, 'to change, to shift'.[16] Hence *t'ugs-rje-ba* and *sñin-rje-ba* mean, in all probability, 'heart-shifting' (from oneself to another or, perhaps, from indifference to active pity). This fundamental meaning is clearly not far removed from the 'will-turning' of Tocharian and would be identical with it if we could be sure that the 'will' of SSS may be interpreted more properly as 'heart, mind'. 'Heart-turning' is better than 'will-turning' as a description of 'pity'. Perhaps the difference between *āriñc* and *kri* (stem *käryā-*) is not so much that between 'heart' and 'will' as the similar, but not identical, one between 'experiencing heart' and 'anticipatory heart'. At any rate it is precisely the parallelism in formation of Tocharian *käryā lotklune* and Tibetan *t'ugs-rje-ba*, *sñin-rje-ba* that enables us to equate *kri*, *käryā-* with *t'ugs* and *sñin* and to give it its proper place in Indo-European. For it is, in all likelihood, a reflex of the Indo-European stem (in *-yā*, f.) which gives us Greek καρδία, Ionic κραδίη 'heart'. The Tocharian word does not represent IE *$\hat{k}\bar{r}d$-$y\acute{a}$ (i.e., *$\hat{k}_{e}rd$-$y\acute{a}$) but *$\hat{k}r_{e}d$-$y\acute{a}$ (reduced from the basic *$\hat{k}red$- seen in Sanskrit *śrad-* and Latin *crēdō < *kred^z-dō*). The former would have given Tocharian A *$k\ddot{a}rci$ (*-d-* preserved as *-t-* after *r*, *l*, *n*; *-t-* palatalized to *-c-* before *e* or *y*; final *-yā* reduced to *-i*), while the latter develops to *kri*, *käryā-* (*-d-* disappears after IE vowels, cf. Toch. *pe* 'foot', dual *pe-m*; *-$_{e}y$- > -y-, -i*).[17] In other words, the present meanings of Tocharian *kri* (*käryā-*) are probably specialized from an archaic meaning 'heart', now rendered *āriñc*, a word of far from obvious etymology. It is likely that at the time of the influence exerted by Tibetan on Tocharian in the creation of the term *käryā lotklune* for 'pity', the stem (perhaps also word) *käryā(-)* still had its primary meaning of 'heart'. If this is true, that influence must have taken place long before the period of the writing down of our Tocharian A texts.

The syntactic use of Tocharian *käryā lotklune* is parallel to that of the Tibetan words to which it corresponds. An example cited in note 9, 'pity for that woman', has 'woman' (*k$_u$li*, obl. *k$_u$le*) in the locative (*k$_u$ley-am*). With this are to be compared analogous Tibetan examples

[16] Jäschke, A Tibetan-English Dictionary 180.

[17] The treatment of IE *d* in Tocharian is a problem which I hope to consider at another time.

with locative (or 'dative') postposition *-la* 'in, at, to', e.g., *mi-la sñiṅ-rje sgom-pa* 'to pity a person', lit., 'person-to heart-shift to-produce'.[18]

3. TOCHARIAN 'ELEPHANT'

The Tocharian A word for 'elephant', *oṅkaläm*, occurs in that form in the nominative singular, further in the genitive as *oṅkälme*. It also occurs in the plural: nom. *oṅkälmāñ*, oblique *oṅkälmās*, and secondary cases (gen., instrum., and *ā*-case) based on the oblique plural.[19] The *-a-* of the nominative, in spite of the fact that it is reduced to *-ä-* in the other cases, looks as though it might be the old thematic IE *-o-* of compounds still found in such cases as *atr-a-tampe* 'provided with hero-might' (: *aträ* 'hero') and *kāsw-a-pälskāñ* 'thoughts of good' (: *kāsu* 'good').[20] If we analyze as *oṅk-a-läm, oṅk-ä-lm-*, it becomes a transparent possibility that this strange word is compounded of *oṅk* 'man' and *läm-* 'to sit' (suppletive to *ṣäm-*; pret. *lyäm, lym-ā-, lam-a-,* subj. *lam-a-,* verbal substantive *lm-ā-lune,* and in derived causative forms[21]), whence *lam-e* 'position' < 'sitting-place'. *oṅk-a-läm* can be understood either as a simple bahuvrīhi, 'having, holding a man's sitting-place' (with *-läm* referable to the noun *lame*), or, perhaps m ore probably (pl. stem *-lmā-*: vb. stem *lmā-*), as a more involved type of bahuvrīhi based on an underlying 'the man sits', hence 'having a man sitting (on him)' (with *-läm* directly referable to the verb stem).[22] The Tocharian material is too fragmentary to allow of certainty in the formal analysis. The probability that *oṅkaläm* is a compound is perhaps enhanced by the fact that its genitive is in *-e*; cf. gen. *ptā-ñkt-e* 'Buddha-god', but *ñäkt-es* 'god' (gen.).[23]

I can find no Sanskrit or Tibetan model for the Tocharian word. Nevertheless, in an indirect way, Tibetan may be presumed to have influenced Tocharian—culturally rather than linguistically, in the strict

[18] Jäschke, A Tibetan-English Dictionary 198.

[19] SSS §237.

[20] SSS §363a.

[21] SSS 475–6.

[22] Somewhat comparable would seem to be such untypical Sanskrit examples as *putra-hata-* 'whose son is killed', *a-danta-jāta-* 'whose teeth have not come to be' (Wackernagel, Altindische Grammatik 2.1.302), except that in these the second element is a verbal derivative (participle) instead of the underlying verb stem.

[23] SSS §123.

sense—in its description of an elephant as an animal used for riding. The Tibetan word for 'elephant' is *glaṅ*, which, according to Jäschke,[24] means also 'ox, bullock'. That the elephant is conceived of in Tibetan as functionally equivalent to a bullock is indicated by the words *glaṅ-po-č'e*, *glaṅ-č'en*, literally 'big *glaṅ*', meaning 'elephant'. But an elephant is not exactly a 'big ox', unless by 'ox' is meant 'beast of burden' or 'riding animal'. That *glaṅ* properly meant this at one time is clinched by the Tocharian A *klaṅk*, B *kleṅke* 'Reittier', obviously related by borrowing to *glaṅ*. Tocharian allows of no final or intervocalic *ṅ* [ŋ], for this consonant, as in most Indo-European languages, is not a true phoneme but merely an assimilated form of *n*. Hence Tibetan *glaṅ*, if Tibetan is the source of the borrowing and not the borrower, was heard as **glaṅg* [or was this a more archaic Tibetan form at the time of the Tocharian borrowing?], whence necessarily **klaṅka* for Old Tocharian. IE *-os* appears as Old Toch. *-a*, whence A zero, B *-e*; IE *o* > *a* > B *e* is regular. SSS offer no proof that *klaṅk* is referable to verb stems *klāṅk-* and *kläṅk-* (SSS 436), whose meanings they are unable to give.[24a] Should it appear that Tocharian *klaṅk* is a native word derived, say, from a verb 'to ride', we would still probably have to assume a historical relation between the Tocharian and Tibetan words. Inasmuch as Tibetan possesses *kl-* as well as *gl-*, a Tibetan borrowing would prove the existence in Tocharian at one time of both *g* and *k*. This would not be surprising in view of the fact that IE *d* and *t* have different reflexes in Tocharian.

Incidentally, Tocharian *klaṅk* proves, if proof were still needed, that such Tibetan initial consonant clusters as *gl-*, *sr-*, and so on, must be taken seriously in spite of the testimony of most of the modern dialects. The classical Tibetan *gl-* is simplified to *l-* in Lahul, Spiti, Tsang-Ü, while in Khams and Balti it appears spirantized to *γl-*.[25] It will appear in the sequel that Tocharian is not without value for the earlier history of Tibetan.

[24] A Tibetan-English Dictionary 80.

[24a] *klāṅklye* (TS 264a2) is a nom. m. pl. of the verbal adjective **klāṅkäl*, from a verb *klāṅk-*. Inasmuch as the form is preceded by *yₐkañ oṅkälmäñ w*. [read *wu* ?] 'horses (and) elephants two [?]', it seems reasonable to connect the word with *klaṅk*. The passage may therefore read: 'Horses (and) elephants, two (sorts) to be used for riding'. In that case *klāṅk-* would either be a denominative verb based on *klaṅk* or, far less likely, *klaṅk* a noun derivative (with IE *o*-vocalism) based on the verb.

[25] Jäschke, A Tibetan-English Dictionary XVIII, sub *glog*.

4. TOCHARIAN ABSOLUTIVES

One of the most characteristic features of Tocharian syntax is the free use of what SSS call 'absolutives', better known as 'gerunds' or 'gerundials'. These are secondary case forms, chiefly ablatives (A, B), instrumentals (B) and *ā*-cases (A) of substantivized preterit participles in -*r*, the participles themselves (nom. m. sing.) ending in A in -*u* or -*o* (contracted from stem in -*ā*- + participial -*u*). In A the ablatives in -*äṣ* are far more common than the *ā*-case forms; in B the corresponding forms are ablatives in -*meṃ* and instrumentals in -*sa*. The actual forms of the absolutives are, therefore; A -*u-r-äṣ* (-*o-r-äṣ*), -*u-r-ā* (-*o-r-ā*), B -*o-r* (-*a-r*, -*e-r*)-*meṃ*, -*o-r-sa*. A literal etymological rendering of such an A form as *wawuräṣ* would be 'from having-given [*or* having-been-given]-ness', an ablative in -*äṣ* of an abstract noun in -*r* based on the preterit participle *wawu* of the reduplicated stem *waw*- corresponding to the indicative preterit stem *wäs*-, *ws*- 'to give', in suppletion to the verb stem *e*-, *āy*-, present *e-s*-.[26]

SSS point out, perhaps correctly, the etymological identity of the -*r*- of these absolutives with the well-known IE neuter -*r* (originally alternating with -*n*-), used to form verbal abstracts and of which a few survivals seem to remain in Tocharian, e.g., A (and B) *ok-a-r* 'growth, plant' (: *ok*- 'to grow'), A *kury-a-r*, B *kary-o-r* 'trade' (: B *käry*- 'to buy'), A *kärs-o-r* 'knowledge' (: *kärs*- 'to know'), *tärk-o-r* 'leave' (: *tärk*- 'to permit').[27] Such examples as A *kärsor* (: pret. part. *kärs-o*) and *tärkor* (: pret. part. *tärk-o*) and B *āyor* 'gift' (: pret part. *āyo*) indicate that while the old IE stock of nouns in -*r* (such as Gk. πῖαρ 'fat', Lat. *iter*) had originally no connection whatever with the IE perfect participle (*-wóns*, *-us*-) which may lie back of the Tocharian forms in -*u* and -*o*, there was in Old Tocharian (i.e., that form of speech from which A and B diverged) a well-established tendency for an *r*-abstract to attach itself to this form, presumably to an old oblique neuter singular in *-u*, *-o* (cf. nom. m. sing. -*u*, -*o*, obl. -*unt*, -*ont*, nom. f. sing. -*us*, -*os*, obl. -*usāṃ*, -*osāṃ*; SSS §257). It may be that the old distribution of oblique neuter forms of this type of verbal abstracts was: absolute -*u* (-*o*), with lost -*r*, but -*ur*- (-*or*-), with retained -*r*, before secondary case endings (e.g., -*uräṣ*, -*urā*). An old -*u*, -*o*: -*ur*-, -*or*- paradigm could level out to -*u*, -*o*: -*u(w)*-, -*o(w)*- or to -*ur*, -*or*: -*ur*-, -*or*-. The latter type of leveling seems to be represented by such words as *kärsor* and *tärkor*. SSS give two

[26] SSS 424. For absolutives see SSS §421 d.
[27] SSS §8.

interesting examples in A of verbal nouns in -*u*, with inflected forms in -*ur*-: *lyalypu* 'what has been left over = *karman*' (substantivized pret. part. of causative of *lip*- 'to remain over'[28]), abl. *lyalypur-äṣ*; and *watku* 'command' (originally unreduplicated substantivized pret. part. of *wätk*- 'to command', cf. regular part. *wotku* < *wawtk-u and its corresponding absolutive *wotkuräṣ*[29]), *ā*-case *watkur-ā*. These forms seem to be archaic and to represent an earlier stratum than *kärsor* and *tärkor*. It is interesting to note that B has an analogical form in -*r* to correspond to A *watku*, namely *yaitkor* (< *wewtka-u[-*r*][30]).[31]

It seems, then, that we have in Tocharian three strata of verbal abstracts in -*r*: (1) a type perhaps directly derived from IE neuters in -*r*; (2) a type of *r*-nouns made over from old perfect participles (e.g., A *watku, watkur*-, B *yaitko-r*, A *kärso-r*); (3) absolutives, in various secondary case forms, based on type 2. Type 3 came to be attached mechanically to every verbal paradigm and so we have such discrepancies as *watku*(*r*-) : *wotku-r-äṣ*. The surprisingly small number, if any, of old *r*-nouns coupled with the creativeness of the *r*-forms in absolutives; the attachment of these formations to the perfect participle; the levelling out of an old -*u* : -*ur*- paradigm to a paradigm in -*ur* : -*ur*-, itself a formation that seems no longer productive in our recorded Tocharian; and the enormous spread of the absolutives in -*u-r-äṣ* and related forms conspire to indicate that a long period of time must have elapsed before the emergence of the Tocharian absolutives as we know them in the texts.

From the strictly Indo-European standpoint it is tempting to attach the Tocharian verbal abstracts in *-*u-r*, -*o-r* and in -*u* : -*ur*- to IE verbal nouns in -*wṛ* (e.g., Hittite forms in -*war* : supines in -*wan*); in that case such a B form as *yaitkor* might be supposed to stem directly from an IE type: pre-Toch. *wéwtk-wṛ. We cannot prove this, however, and in any event it has to be shown why this old IE pattern, if it be such in Tocharian, was worked into the preterit system of the verb paradigm. The most plausible standpoint would be to assume that when the treatment of final syllables in Tocharian had reached the point at which IE *-*wóns* had worn down to Toch. -*u*, the old perfect parti-

[28] SSS §68 and p. 466.
[29] SSS §68 and p. 469.
[30] Original -*e*- of reduplicating syllables becomes palatalizing -*a*- in A and B. Palatalized *w*, say *wᵛ* or *ẅ*, reverts to *w* in A but becomes *y* in B; another example is A *want* 'wind': B *yente*.
[31] SSS §8.

ciple tended to become confused with the old r-neuter in $-u$ ($:$ $-ur$-)
derived from IE $*$-$w\underset{.}{r}$. We would still have to explain why such a fusion
of forms was semantically possible.

I shall try to show that the Tocharian 'preteritizing' of IE verbal
abstracts in $*$-$w\underset{.}{r}$[32] and the form and syntax of Tocharian absolutives are
all due to Tibetan influence. It will be well to give a few examples of
these absolutives from the Tocharian texts:

A: *cesmäk puk śtwar śälkās pokeyo wawuräṣ poñcäs kosām tāpam*[33]
śkaṃ lo 'eben-jene alle vier, Schläge (?) mit-der-Tatze ausgeteilt-
habend, sämtlich erschlug-er-sie frass-sie und auf'[34]

wrasañ cam peke pälkoräṣ yneś pälskaṃ yāmuṣ '(die-)Menschen, dies
Gemälde nachdem-(sie-)gesehen, für-Wirklichkeit im-Geiste die-(es-)-
gehalten-haben [nom. m. pl. of part.]'[35]

*käntantuyo wältsantuyo tmānantuyo korisyo waṣtäṣ lanturäṣ ... kälp-
nānträ* 'zu-Hunderten, Tausenden, Zehntausenden, Koṭi's aus-dem-
Hause nachdem-(sie-)gegangen-waren ... erlangen-sie'[36]

B: *mant eṅkor-meṃ weñāmeś* 'so (ihn-)ergriffen-habend, sprach-er-zu-
ihnen'[37]

keklyauṣor-meṃ mrauskāte 'ayant-entendu il-prend-en-dégout-le-
monde'[38]

In all these cases the absolutive (*wawur-äṣ* 'having given', *pälko-räṣ*
'having seen', *lantu-räṣ* 'having gone', B *eṅkor-meṃ* 'having got hold of',
keklyauṣor-meṃ 'having heard') indicates the priority of an event to the
event expressed by the main verb or equivalent therefor, such as a
participle (*kosām* 'he caused them to die', *yāmuṣ* '[they] who have made',
kälpnānträ 'they attain', B *weñāmeś* 'he spoke to them', *mrauskāte* 'he
became disgusted with the world'). The subjects of the two verb forms

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[32] Alternatively, the direct attachment of verbal abstracts in $-r$ to the reflex of
the IE perfect participle (or other prototype of Toch. $-u$, $-o$).

[33] Corrected by SSS from *tāpap*.

[34] See E. Sieg, 'Die Geschichte von den Löwenmachern in tocharischer Version'
(in Aufsätze zur Kultur- und Sprachgeschichte vornehmlich des Orients Ernst
Kuhn gewidmet 149, 151 [1916]). I have very slightly rearranged Sieg's transla-
tions so as to let the German version reflect the Tocharian original as literally as
possible.

[35] T(ocharische) S(prachreste) 9 a 2; SSS 271.

[36] TS 254 b 7; SSS 198. Better, presumably, 'gegangen sind'.

[37] E. Sieg und W. Siegling, 'Die Speisung des Bodhisattva vor der Erleuchtung'
(Asia Major 2. 280, 283 [1935]).

[38] S. Lévi, Fragments de Textes Koutchéens 73–4 (1933). Better, presumably,
'il prit en dégout'; cf. A *mroskat* (pret. med., 3d pers. sing., of *mrosk-* '[der Welt]
überdrüssig werden'), SSS 457.

are the same but there is no true expression of person in the absolutive. It is a purely impersonal or generalized reference to an event which is set in prior relation to the main event by the use of an ablative case suffix.

If, now, we turn to Tibetan, we find that syntactically parallel forms, i.e., case-forms, among others ablatives, of the generalized or non-personal verb,[39] often in its perfect form, are found, and with identical function. Thus, to the five Tocharian absolutives illustrated above correspond the following Tibetan forms: *bskur-nas* 'after giving, having given' (*bskur*, perf. of *skur-ba* 'to give'; *-nas*, postposition with ablative force, 'from'), *mt'oṅ-nas* 'after seeing, having seen' (*mt'oṅ-ba* 'to see' has no separate perfect), *soṅ-nas* 'after going, having gone' (*soṅ*, perf. of ₒ*gro-ba* 'to go'), (*b*)*zuṅ-nas* 'after taking hold, having taken hold' ((*b*)*zuṅ*, perf. of ₒ*dzin-pa* 'to take hold'), *t'os-nas* 'after hearing, having heard' (*t'os-pa* 'to hear' has no separate perfect).

I shall give a few examples from Jäschke of Tibetan gerundial constructions in *-nas* 'from':

    dei ts'ig-gis bskul-nas 'that-of word-by induced [perf. of *skul-ba* "to exhort, admonish"]-from' = 'induced by his words'[40]

    rnam-šes las daṅ nyoṅ-moṅs-kyis bskul-nas 'part-know [= "vijñāna, soul"; *rnam-šes-pa* "to know fully"] work together-with misery-by induced-from' = 'the (departed) soul urged on, influenced, driven, by its former works and sins'[41]

    lhuṅ-zed nam-mk'a-la bskyur-nas 'alms-bowl sky-to thrown [perf. of *skyur-ba* "to throw, cast", fut. *bskyur*]-from' = 'having thrown his mendicant's bowl up into the air'[42]

    me-tog gtor-nas žus-pa 'flower strewn [perf. of *gtor-ba* "to strew, scatter"]-from spoken [perf. of *žu-ba* "to speak to a person of higher rank"] -the' = 'after having strewn flowers, they said humbly]'[43]

[39] Properly speaking, the Tibetan verb has no inherent implication of person. All transitives, moreover, are best understood as inherently passive. It has never been made clear, so far as I can see, when the Tibetan verb is tenseless and when it has a set of tense-mode forms (present, perfect, future, imperative); thus, *mgu-ba* 'to rejoice' and *bgrod-pa* 'to walk' are used in all tenses and modes, whereas *sdod-pa* 'to sit' has a specific perfect-future form *bsdad* and ₒ*god-pa* 'to design' ('present' or infinitive) has a perfect (*bgod*), a future (*dgod*), and an imperative (*k'od*).

[40] Jäschke, A Tibetan-English Dictionary 23.
[41] Ibid.
[42] Op. cit. 27.
[43] Jäschke, Tibetan Grammar 157.

nam laṅs-nas soṅ 'night arisen [perf. of *laṅ-ba* "to rise", imperative *loṅ, loṅs*]-from went [perf. of *ₒgro-ba* "to go", imperative *soṅ*]' = 'when the night had risen [= at daybreak] he went'[44]

laṅ-nas soṅ 'arise-from go' = 'after you will have risen, go!'[45]

de mt'oṅ-nas skad p'yuṅ-ste ṅus̓-so 'that seen-from noise caused-to-come-forth [perf. of *ₒbyin-pa*, fut. *dbyuṅ*, imperative *p'yuṅ*; caus. of *byuṅ-ba* "to come out"]-(gerund) wept-(period)' = 'when I saw that, raising clamor, I wept'[46]

The resemblance of the syntax of these sentences to that of the Tocharian examples is obvious. In both Tocharian and Tibetan the gerund precedes; has an implied, never expressed, subject (or agent) which is generally identical with the subject (or agent) of the main verb; is attached to the perfect participle of the verb (the Tibetan perfect is best interpreted as a participle and, when transitive, as a passive participle); and is a case form, often ablative, of the verbal noun. There can be little doubt that such Tibetan (or linguistically related) models as these are responsible for the essentially un-Indo-European absolutives of Tocharian. The Tibetan forms in *-nas* are only one type of gerund, of which Francke lists no less than eleven, most of them case forms[47]: *-de, -te, -ste* 'after, and'; *-čiṅ, -šiṅ, -žin* 'when, and'; *-kyin, -gin, -gyin, -'in, -yin* [gen. + *-n*] 'when, and'; *-kyi, -gi, -gyi, -'i, -yi* [gen.] 'but, though'; *-kyis, -gis, -gyis, -'is, -yis* [instrumental] 'because, as; but, though'; *-pa-s, -ba-s* [instr. of infinitive] 'because'; *-la* [dat.]; *-na* [loc.] 'when, if; as, because'; *-nas* [abl.] 'after'; *-las* [abl.] 'while' (added to inf.); *-pa-r* [terminal of inf.] 'that, and'. Until we know more about Tocharian syntax than we do, we shall not be able to state definitely what are the Tibetan prototypes of the A absolutives in *-ā* and the B absolutives in *-or-sa*. Formally, A forms in *-ur-ā, -or-ā* probably correspond to Tibetan gerunds in *-pa-r* (*-tu, -du, -ru, -r*: terminal or allative case suffix; Toch. *-ā* may be an old terminal [< IE *ad* 'to'?] which combines instrumental and modal uses), B forms in *-or-sa* (with instrumental *-sa*) to Tibetan instrumental gerunds in *-kyis* and *-pa-s* (note parallelism of Tib. *-pa-s*, nominalizing or 'infinitive' suffix + instrumental case suffix, to Toch. B *-or-sa*, verbal noun suffix of pret. + instrumental case suffix). A forms in *-ur-äṣ, -or-äṣ* and B forms in

[44] Op. cit. 57.
[45] Ibid.
[46] Ibid.
[47] Op. cit. 155-8; see also 54-64.

-or-meṃ, as we have seen, correspond closely, in form and function, to Tibetan gerunds in *-nas*.

In further numbers even more far-reaching Tibetan influences will be traced.   These influences are by no means confined to general syntactic procedures, as we shall see, but embrace a large number of lexical borrowings, some of them of grammatical importance, and morphological transfers.   The phonology of Tocharian, a notoriously difficult field, receives abundant light from the treatment of Tibetan loan-words, and we shall see in the end that it is precisely the unanalyzed Tibetan element in Tocharian which has prevented us from arriving at a true notion of the placement of Tocharian in the Indo-European group of languages.   We shall also find that it is quite possible to infer the dialectic zone within the vast Tibetan-speaking area to which the Tibetan influence on Tocharian is to be credited.

PROSKYNESIS AND ADORARE

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The verb προσκυνεῖν, which is commonly used to describe the worship of the gods and the obeisance before rulers, has been the subject of several recent discussions in connection with its use in the New Testament and in the literature of the imperial cult. In a paper on Alexander, Schnabel,[1] relying chiefly on the etymological meaning of the word, explained the *proskynesis* at Bactra as a kiss sent to the daimon of Alexander. This view was adopted by some scholars.[2] But another interpretation of προσκυνεῖν has recently found favor. Bolkestein, in his study of the *Deisidaimonia* in Theophrastos,[3] has attempted to show that the gesture implied in προσκυνεῖν was to kneel and not to send a kiss. *Proskynesis*, he says, is generally supposed to have had two distinct meanings, the first one implying a hand-kissing gesture, the second indicating the Oriental obeisance. But, Bolkestein (30) continues, a careful examination of the texts shows that, although προσκυνεῖν originally meant 'zu Fuss fallen und küssen', in Greek literature it is used only for a kneeling gesture; if the hand-kissing gesture was known in the religion of ancient and classical Greece it was never called *proskynesis*. The result of Bolkestein's study may be seen in Nock's statement[4] that 'kneeling is the only meaning of the word, as Bolkestein has shown.' In his admirable article on the ceremonial of the Imperial court, Alföldi[5] accepts Bolkestein's interpretation. He calls Berve's explanation[6] of

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[1] Klio 19.116–18 (1925).

[2] G. Radet, Alexandre le Grand 257–68; Paris, 1931. L. R. Taylor, The Divinity of the Roman Emperor, in Am. Phil. Ass. Monogr. 1, App. 2 (1931). Berve, Klio 20.182 (1926), rejects Schnabel's interpretation of the *proskynesis* at Bactra, but gives the hand-kissing gesture as one meaning of *proskynesis*.

[3] Theophrastos' Charakter der Deisidaimonia, in Religionsgeschichtliche Versuche und Vorarbeiten 21.2.21–30 (1929).

[4] Gnomon 8.157 (1932).

[5] Die Ausgestaltung des monarchischen Zeremoniells am röm. Kaiserhofe, in Mitt. des deutsch. Arch. Inst. Röm. Abt. 49.1–119 (1934).

[6] Klio 20.182, n. 2 (1926).

προσκυνεῖν ὡς θεὸν τὸν 'Αλέξανδρον as a hand-kissing gesture 'ein Irrtum, verursacht durch die seither durch Bolkestein widerlegte übliche Erklärung der griechischen Proskynese.'[7]   In Horst's recent study of the verb προσκυνεῖν,[8] the gesture implied, especially in the classical period, is not discussed in detail, though Horst does not entirely accept Bolkestein's results.

The interpretation of προσκυνεῖν as implying a kneeling gesture only is too dogmatic, especially when stated by those who follow Bolkestein; he himself recognises some difficulty in interpreting some passages:

τὰ τοῖν ποδοῖν ἴχνη καταφιλεῖν[9] for instance shows that the similar phrase τὸ ἴχνος προσκυνήσομεν[10] indicates a kissing if not a hand-kissing gesture. Bolkestein also mentions passages which might imply a kissing gesture and others that seem doubtful.[11]   But there is other evidence not cited by him which shows that in most cases when προσκυνεῖν was used for the worship of the gods the gesture implied was kissing and not kneeling.   I have therefore brought together in this paper material on the use of προσκυνεῖν; I have considered also the Latin verb *adorare*, which was generally used to translate προσκυνεῖν and which was associated with a hand-kissing gesture.   This new examination of the evidence may be of some value to those interested in the history of ritual.

Modern etymologists are agreed that προσκυνεῖν is a compound of the verb κυνεῖν, originally 'to kiss', with the prefix πρός, which in composition means 'toward, before, in addition to'.   We need not go into details, as the evidence is fully and adequately presented by Horst.[12]   Thus, since this verb, meaning 'to worship' or 'to greet respectfully', is formed by a combination of κυνεῖν 'to kiss' and the prefix πρός, its original meaning would a priori seem to have been 'to worship' or 'to greet with a kiss'; the prefix πρός would indicate the direction toward which the kiss was sent.   Let us now examine the texts in order to see whether the verb actually did imply a gesture, and if so what the gesture was.

There are from the post-Christian period definite indications of the association of προσκυνεῖν with a hand-kissing gesture.   For instance a

[7] 14, n. 5.

[8] Proskynein.   Zur Anbetung im Urchristentum nach ihrer religionsgeschicht-lichen Eigenart (Neutestamentliche Forschungen 3.2); Gütersloh, 1932.

[9] Alciphron 3.67.1.

[10] Philostratus, Ep. 18.

[11] 31, n.3; n.7.

[12] 10–14.   See also Friedrich Schwenn's review of Horst, in Gnomon 11.479-83 (1935).

late definition of the word, that of Hesychius,[13] describes a hand-kissing gesture: ἔνια τῶν βαρβάρων ἐθνῶν τοὺς ἀντίχειρας ὑποτιθέντα τοῖς γενείοις καὶ τοὺς δακτύλους ἐκτείνοντα προσκυνεῖ τοὺς ἡγουμένους αὐτῶν. Lucian[14] also connects the gesture with a kiss thrown with the hand. Describing the death of Demosthenes, he says that the orator was in the temple of Poseidon with his guardian, who had been ordered to bring him alive to King Antipater; while pretending to perform *proskynesis* to the god, Demosthenes took poison without arousing his guardian's suspicion for, seeing him 'lift his hand to his mouth' he thought that it was only to perform *proskynesis*. In this case προσκυνεῖν is clearly used to describe a kiss thrown with the hand. Likewise in the passage of Dio,[15] where Caracalla driving his chariot salutes the spectators from the arena with his whip, the phrase προσεκύνει τε αὐτοὺς κάτωθεν τῇ μάστιγι undoubtedly refers to his waving a kiss with the whip held in his right hand. That in such a case the gesture was actually a kiss thrown with the hand is further indicated by the following passages: Phaedrus[16] describes a flute-player saluting the audience: *Iactat basia tibicen; gratulari fautores putat*. Tacitus[17] describes Nero flattering the spectators before one of his performances: *Postremo flexus genu et coetum illum manu veneratus*. Again Martial has the following statement:[18] *Audieris cum grande sophos dum basia iactas*. When Suetonius[19] describes Claudius watching the games: *surgens et ipse cum cetera turba voce ac manu veneratus est*, he probably refers to the same gesture.

These passages, however, are taken from late writers. Let us now examine the meaning of προσκυνεῖν in early Greek literature. Προσκυνεῖν does not appear in Homer or Hesiod. The only instance before Aeschylus is a fragment of the sixth century poet Hipponax,[20] where the etymological meaning is apparent: παρ' ᾧ σὺ λευκόπεπλον ἡμέρην μείνας πρὸς μὲν κυνήσειν τὸν Φλυησίων Ἑρμῆν. Aeschylus uses προσκυνεῖν twice, once in the Persians:[21] γαῖαν οὐρανόν τε προσκυνῶν, and once in the Prometheus Vinctus:[22] οἱ προσκυνοῦντες τὴν Ἀδράστειαν σοφοί. The verb does not

[13] Lexicon s. v. ἀντίχειρε.
[14] Enc. 49.
[15] 77.10.
[16] 5.7.28.
[17] Ann. 16.4.
[18] 1.3.7. Cf. Juv. 4.117; 3.106.
[19] Claud. 12.2.
[20] Frag. 37 Diehl.
[21] 499.
[22] 936.

occur in Pindar, but is frequently found in Sophocles; there are several
instances of it in the Philoctetes[23] and one in each of the following plays:
Electra,[24] Oedipus Tyrannus,[25] Oedipus Coloneus.[26] It occurs in
Herodotus,[27] Euripides,[28] and Aristophanes,[29] but never in Thucydides.
Since the word had not become very common, it would probably keep
its etymological meaning for some time, and fifth-century writers must
still have felt the force of κυνεῖν 'to kiss' in προσκυνεῖν 'to worship, to
salute'.

There is moreover evidence that προσκυνεῖν was used as a hand-kissing
gesture at an early period in its association with Nemesis, often under
the name of the goddess of unavoidable punishment, Adrasteia. The
usual formula for deprecating the wrath of the goddess or the vengence
of the gods for indiscreet words was: προσκυνῶ τὴν Νέμεσιν. This is
found in Aeschylus, Sophocles, and other writers of the classical period.[30]
'I remember that I am a man,' says Demosthenes,[31] 'and I perform
proskynesis to Nemesis.' Plato[32] has 'so I perform proskynesis (προσκυνῶ)
to Nemesis in what I am about to say.' In this case the gesture must
certainly have been a kiss, for in the same connection a poem of the
Greek anthology[33] uses the simple verb κυνεῖν 'to kiss': Ἡ Νέμεσιν
δεινὴν οὐχὶ κύσασα θεόν. Moreover Pliny[34] indicates that a kiss was sent
with the hand when he says that the seat of Nemesis lies behind the ear,
and that 'it is to this part that we apply the little finger after touching
the mouth with it when we silently ask the pardon of the gods for having
let slip an indiscreet word.'

The same gesture is implied by Xenophon also, in a passage[35] where
προσκυνεῖν is associated with a seeze; as he was making a speech to the
soldiers 'a man sneezed, and when the soldiers heard it they all, with one

[23] 1408; 533; 657; 776.
[24] 1374.
[25] 327.
[26] 1654.
[27] 1.134; 2.80; etc.
[28] Tro. 1021; Or. 1507.
[29] Eq. 640; Plut. 771; etc.
[30] Aesch. Prom. 936; Soph. Ph. 776; Menander, Perik. 114; Chariton, 3.8.6 etc.
[31] Adv. Arist. 1.37.
[32] Rep. 451a.
[33] 6.283.2.
[34] N. H. 11.251: *est post aurem aeque dexteram Nemeseos, quae dea Latinum
nomen ne in Capitolio quidem invenit, quo referimus tactum ore proximum a minimo
digitum veniam sermonis a diis ibi recondentes.*
[35] Anab. 3.2.9; cf. Aristophanes, Eq. 640.

accord, performed *proskynesis* to the god.' In this passage also, Bolke-
stein's interpretation is unsatisfactory, for it is most unlikely that the
whole army knelt or even kissed the earth at the sound of the sneeze.
The custom of performing *proskynesis* to the omen of sneezing is men-
tioned by Aristotle:[36] ὥστε ὡς σημεῖον ὑγείας τοῦ ἀρρώστου καὶ ἱερωτάτου
τόπου προσκυνοῦσιν ὡς ἱερὸν καὶ φήμην ἀγαθὴν ποιοῦνται. Pliny says[37] that
men are saluted when they sneeze, but does not explain of what the
salutation consists; he may mean merely such an utterance as *salve*;
however, when he says that the emperor Tiberius insisted on being thus
saluted 'even when he was driving in his chariot', he seems to imply some
definite gesture, probably a kiss sent with the hand. Moreover Athe-
naeus[38] proves the statement that the head was sacred by the fact that
'men performed *proskynesis* to the sneezes that came from it as if they
were sacred.' There again some gesture must have been implied.

There are other passages in Greek literature where προσκυνεῖν obvi-
ously describes a gesture and where kneeling is impossible. Plutarch,[39]
for instance, has an interesting description of Marcellus doing *proskynesis*
to the Sun. When his horse had wheeled about at the sight of the enemy
he was afraid that this might be interpreted as a bad omen by the
soldiers. Therefore 'he reined his horse round to the left and made him
face the enemy while he himself performed *proskynesis* to the Sun.'
There is clearly no possibility for kneeling here, and the gesture which
was to be seen by the whole army can hardly have been anything but a
kiss thrown with the hand toward the sun.

Classical writers use προσκυνεῖν in connection with the sun at a period
when the meaning to kiss was still uppermost; we have therefore ample
justification for assuming a hand-kissing gesture in phrases such as καὶ
προσκυνῶ γε πρῶτα μὲν τὸν Ἥλιον from Aristophanes' Plutus;[40] προσκυλίσεις
ἅμα καὶ προσκυνήσεις ἀκούοντές τε καὶ ὁρῶντες Ἑλλήνων τε καὶ βαρβάρων
from Plato;[41] ἥλιε, σὲ γὰρ δεῖ προσκυνεῖν πρῶτον θεῶν from Menander,[42]
and πᾶς προσκυνεῖ δὲ τὸν στρέφοντα κύκλον ἡλίου from Sophocles.[43] Indeed

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[36] Problemata 962.38b (= 33.9.3).

[37] N. H. 28.23: *Cur sternuentis salutamus?*

[38] Deipn. 2.66c: ὅτι δ' ἱερὸν ἐνόμιζον τὴν κεφαλὴν δῆλον ἐκ τοῦ καὶ κατ' αὐτῆς ὀμνύειν
καὶ τοὺς γινομένους ἀπ' αὐτῆς πταρμοὺς προσκυνεῖν ὡς ἱερούς.

[39] Marc. 6.6.

[40] 771.

[41] Leg. 887e.

[42] Frag. 609 ed. Kock.

[43] Frag. 672 Nauck².

when Arrian[44] represents Callisthenes explaining to the Greeks that gods and men are honored in different ways, he says: 'Men are kissed by those who salute them; but as for the divine beings, because they are far above us and it is not even right to touch them, they are honored by our performing *proskynesis* to them.' Is this not a clear indication that *proskynesis* to a god was a substitute for a kiss of the god, the kiss being waved to him because it was not right to touch him? Again Lucian[45] states quite definitely that it was the custom of the Greeks to kiss their hands to the sun in the morning: ὅπου καὶ Ἰνδοὶ ἐπειδὰν ἔωθεν ἀναστάντες προσεύχωνται τὸν Ἥλιον, οὐχ ὥσπερ ἡμεῖς τὴν χεῖρα κύσαντες ἡγούμεθα ἐντελῆ ἡμῶν εἶναι τὴν εὐχήν. Thus the celestial divinities and others were worshipped with a kiss sent toward the sky or toward their statues.

The earth goddess, however, was worshipped with a different gesture. As is natural, the custom when adoring Ge was not to send a kiss toward the Earth, but to bow down and kiss the Earth itself. Thus Agamemnon kissed his native soil when he came back,[46] and Ulysses when he landed 'laid himself down among the rushes and kissed the bounteous earth.'[47] Homer describing this attitude uses the verb κυνεῖν, and a later scholiast explains it by using προσκυνεῖν.[48] When Plutarch[49] describes men doing *proskynesis* 'as if they had just caught sight of the land' from the sea, he surely refers to this custom and implies that the gesture used to salute the land still far away was a kiss sent with the hand. Since there is a well-attested custom of bowing down and kissing the earth at an early period, there is no reason to assume that προσκυνεῖν was used metaphorically when associated with Ge. Thus in Aristophanes ἔπειτα τὴν γῆν πρόσκυσον καὶ τοὺς θεούς,[50] a kiss of the earth is the gesture described by προσκυνεῖν, and also in Polybius,[51] when he describes the Carthaginian envoys at a much later date: τοὺς θεοὺς ἀσπάσαιντο καὶ τὴν γῆν προσκυνήσαιεν. When *proskynesis* was performed both to the earth and to the celestial divinities a double gesture is to be understood, the worshipper throwing a kiss to the sky and then bowing down to kiss the earth. This is the case in the Oedipus Coloneus,[52] where a worship-

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[44] Anab. 4.11.3.
[45] Salt. 17.
[46] Odys. 4.522.
[47] Odys. 5.463; cf. 13.354.
[48] Ebeling, Lex. Homer. s.v. κυνέω, quoted by Horst 118 (see note 8).
[49] Ant. 49.
[50] Eq. 156.
[51] 15.1.6.
[52] 1654-5: ὁρῶμεν αὐτὸν γῆν τε προσκύνονθ' ἅμα
καὶ τὸν θεῶν Ὄλυμπον ἐν ταὐτῷ λόγῳ.

per is seen 'doing *proskynesis* both to earth and the home of the gods above, together and in one prayer,' and in the Persae,[53] where also *proskynesis* is done to the earth and to the sky.

A similar gesture is the kissing of the threshold of a house or a temple, either as a mark of worship or as a last farewell. Vergil describes the women about to leave Priam's palace:[54]

> *Tum pavidae tectis matres ingentibus errant*
> *Amplexaeque tenent postes atque oscula figunt.*

Ovid shows the Trojans kissing the earth before they depart. In Valerius Flaccus a man about to leave his country kisses the earth: *Ultima tum patriae cedens dedit oscula ripae.*[55] This gesture is probably meant by προσκυνεῖν in Sophocles: 'Come let us depart after having done *proskynesis*',[56] and again, 'Set forth after having done *proskynesis* to this land.'[57]

Finally the interpretation of *proskynesis* as a kissing gesture is strengthened by a comparison of the story of King Prusias' salutation to the Roman senate, as told by Polybius and Livy. In their versions of the same incident, Polybius[58] simply notes that Prusias performed *proskynesis*, whereas Livy[59] gives a detailed description of the king's attitude. According to Polybius, Prusias stood in the doorway facing the members of the senate, and putting both his hands on the ground he performed *proskynesis*. This might fit Bolkestein's interpretation of *proskynesis* as kneeling or prostration if Livy in criticizing Polybius' account had not translated it almost literally; he clearly understood Polybius' use of προσκυνεῖν to mean prostration accompanied by a kiss of the threshold, and rendered it as follows: *summisisse se et osculo limen curiae contigisse.*

In the passages which I have quoted a kissing gesture can almost certainly be proved to have been implied in προσκυνεῖν. There are however cases where it is impossible to decide whether or not such a

[53] 499.
[54] Aen. 2.488; cf. 4.659.
[55] Val. Flaccus 4.373; cf. Ov. Met. 13.421, 3.24, 7.631, etc.
[56] Phil. 533: ἴωμεν, ὦ παῖ, προσκύσαντε τὴν ἔσω.
[57] Ibid. 1408; στεῖχε προσκύσας χθόνα.
Cf. Electra 1374:   πατρῷα προσκύσανθ' ἔδη
        θεῶν, ὅσοιπερ πρόπυλα ναίουσιν τάδε.
[58] 30.16.5.
[59] 45.44.

gesture was indicated. For instance when Camillus falls as he is doing *proskynesis*[60] he cannot possibly have been kneeling, but a prayer may be meant with no accompanying gesture. The fact remains, however, that the Greeks often used προσπίπτειν in connection with προσκυνεῖν to indicate kneeling or prostration; when this is the case, as usually for the obeisance before rulers, προσκυνεῖν is not used metaphorically, but may generally be assumed to indicate a kissing gesture. Thus the verb προσκυνεῖν was used to describe a gesture of reverence of which a kiss was properly a part.

The hand-kissing gesture was known also in the religious ritual of the Romans. A series of definitions and descriptions, all of them from the literature of the Empire, associate with it the verb *adorare*. Thus Pliny[61] says: *In adorando dextram ad osculum referimus totumque corpus circumagimus.* Apuleius[62] gives a graphic description of *adorare* as a hand-kissing gesture: *Et admoventes oribus suis dexteram primore digito in erectum pollicem residente, ut ipsam Venerem prorsus deam religiosis adorationibus venerabantur.* In another passage[63] he says: *Adorandi gratia manum labris admovere.* More than two centuries later Jerome[64] associates *adorare* with a hand-kissing gesture: *qui enim adorant solent deosculari manum.*

An assumption that *adorare* had some connection with *os* 'mouth' may explain why *adorare* (rather than *salutare* or *venerari*, which also are occasionally found) was generally chosen to translate προσκυνεῖν. It is however fairly certain that *adorare* had no etymological connection with the noun *os* and had originally no association with a hand-kissing gesture. This fact can now be definitely established from the full citations of the use of the word in the Thesaurus Linguae Latinae.

Adorare is a compound of the verb *orare* with the prefix *ad.*[65] *Ōrāre* itself had long been associated with *ōs* 'mouth', of which it was taken to be the denominative.[66] One difficulty in accepting this derivation is

[60] Plutarch, Cam. 5: ταῦτ' εἰπών, καθάπερ ἐστὶ 'Ρωμαίοις ἔθος ἐπευξαμένοις καὶ προσκυνήσασιν ἐπὶ δεξιὰ ἐξελίττειν, ἐσφάλη περιστρεφόμενος. Cf. Lucian, Tim. 23: τὸν μυλῶνα ὥσπερ τὸ 'Ανάκτορον προσκυνῶν; Plutarch, De Superstitione 3 (166b): αἰσχρὰς προκαθίσεις, ἀλλοκότους προσκυνήσεις; etc.

[61] N. H. 28.25.

[62] Met. 4.28.

[63] Apol. 56.

[64] I Contra. Ruf. 19.

[65] Ernout and Meillet, Dictionnaire étymologique de la langue latine, s. v. *orare.*

[66] Ibid.; cf. Varro L. L. 6.76: *oro ab ore*; Ennius, Sc. 306 Vahlen²: *quam tibi ex ore orationem duriter dictis dedit.*

that if *ōrāre* is denominative to *ōs*, the cognate Oscan word *urust* 'oraverit, egerit' would have been **uzust*, and its *-r-* could be explained only on the theory that the word as a legal term was borrowed from the Latin, which seems highly improbable.[67] No other word like *ōrāre* has been derived from *ōs*. *Ōrāre* more probably belongs to the group of words meaning to pronounce words of a solemn character, like *dīcere*, *fārī*. The verbal prefix *ad* is often used as an intensive with no idea of motion toward, as for instance in the verbs *adaugeo, adcuro, adformido, adiuro*.[68]

The use of the verb in early Latin literature supports this derivation, for it is never associated with a hand-kissing gesture. Like *orare*, its earlier meaning seems to have been 'causam agere', that is, 'to pronounce solemn words', either a legal or a religious formula. *Ōrare antiquos dixisse pro agere, testimonio sunt et oratores, et i qui nunc legati, tunc vero oratores, quod reipublicae mandatas partis agebant*.[69] Servius[70] gives a similar definition of *adorare*: *Veteres qui adorare adloqui dicebant*.

This word occurs for the first time in Latin literature in the Laws of the Twelve Tables: *Si adorat furto quod nec manifestum erit ...* , where it seems to mean 'to accuse'.[71] The verb *adorare* is not found in Ennius, Plautus, Caesar, Cicero, or Horace, and occurs only rarely in the literature of the Republic, where *venerari* is more commonly used to express worship. The meaning 'to speak with veneration, to worship', was derived from the earlier meaning, 'to pronounce words of a solemn character'. *Adorare* then became a synonym of *colere, venerari*. Our earliest instance of the word in this sense is in Laevius:[72] *Venerem igitur almum adorans, si femina sive mas est ....* It is the usual meaning in Vergil and Livy, but the relation to *orare* was probably still felt, especially when there was a connotation of 'pray, implore'.[73]

[67] Buck, Osc.-Umb. Gram. 21.

[68] Walde, Lat. Etym. Wörterbuch² 548; Stolz-Schmalz, Lat. Gr.⁵ 496. The ancient grammarians were uncertain about the etymology of *adorare*; they never seem to connect it with a hand-kissing gesture. Nonius 52 M would derive it from the word *ador* 'spelt': *ador, frumenti genus, quod epulis et immolationibus sacris pium putatur: unde et adorare, propitiare religiones, potest dictum videri*. Lydus, Mag. 1. 47, derives it from *adorea* 'glory won in war'.

[69] Festus 218.6 Lindsay (= 198b.23M). *Re inorata* in Cicero and Ennius means 'without having been able to plead the case '. Cf. also Quintilian's *ars orandi, orandi scientiam*, etc.; Vergil, Aen. 10.96: *talibus orabat Juno*; Livy 39.40.12; etc.

[70] Ad. Aen. 10.677.

[71] Festus 158 Lindsay (= 162b.19 M).

[72] Frag. 26; in Baehrens, Frag. Poet. Rom.

[73] Livy 38.43.5: *deos ... quos adorent ... ad quos precentur et supplicent*; 7.40.4: *deos immortales ... ita adoravi ... ut mihi ... gloriam ... darent*; Ovid, Tr. 1.3.41: *hac prece adoravi ...* ; etc.

Thus there was originally no relation between *adorare* and a gesture of worship. To describe a kiss waved with the hand in adoration of a god, the Romans used phrases such as *basia iactare* or *iacere*, *oscula iactare*, *manu venerari*. To translate προσκυνεῖν they often used a whole phrase, like Livy's *summisisse se et osculo limen contigisse*.[74]

In the literature of the Republic and of the beginning of the Empire, the verb *adorare* was not used to describe the obeisance before rulers. Cornelius Nepos[75] makes a Persian official explain this custom to Conon: *Necesse est enim, si in conspectum veneris, venerari te regem (quod προσκύνησιν illi vocant), nemo enim sine hoc admittitur*. Valerius Maximus,[76] writing under Tiberius, has *adulare* and *salutare*: *more Persarum adulata ... salutavit*. Curtius Rufus, in the reign of Claudius, uses *adorare* for the worship of the gods; when describing the Persian obeisance he uses *venerari*.[77] For instance, the famous incident of the Persian courtier doing *proskynesis* to Alexander and being mocked by one of the Macedonians is reported in the following terms: *Quem venerantibus Persis Polypercon ... unum ex his, mento contigentem humum per ludibrium coepit hortari ut vehementius id quateret ad terram ...* Whereupon Alexander said to the Macedonian courtier: *Non veneraberis me?*

Eventually *adorare* came to be the word regularly used for the obeisance and also, as we have seen, for worship and salutation expressed by means of a kiss. The earliest use of it in this sense is the passage already quoted from the Elder Pliny. Tacitus[78] so uses the word in describing Otho before his election: *nec deerat Otho protendens manus adorare vulgum, iacere oscula et omnia serviliter pro dominatione*. The clause *protendens manus adorare vulgus* is explained by *iacere*

[74] 45.44.

[75] Conon 3.3.

[76] 4.7. ext. 2; cf. also 6.3. ext. 2.

[77] 8.5.22; cf. also 3.12.17: *suo more veneratae sunt*; 5.2.22: *procumbens venerari me*; 10.5.33: *venerari ipsum*. See also Livy 9.18.4: *referre in tanto rege piget superbam mutationem vestis et desideratas humi iacentium adulationes*. Justin, in his version of Trogus Pompey's Histories written under Augustus, says (Epit. 1.10): *Dareum regem salutaverint*. *Salutare* is also used by Curtius 8.5.6: *salutare prosternentes humi corpora*. In another passage of Trogus, however, the verbs *adorare* and *salutare* are contrasted, *adorare* being used for the obeisance (Epit. 12.7.1): *non salutari sed adorari se iubet*. This use of *adorare* may of course be an addition of Justin's, the author of the Epitome, who lived under the Antonines.

[78] Hist. 1.36; cf. Dio 64.8; φιλήματά τε ὡς ἑκάστοις διὰ τῶν δακτύλων ἔπεμπε. *Adorare* seems to imply a kissing gesture in the following: Suetonius, Nero 12.3: *coronam adoravit*; Juvenal 6.47: *Tarpeium limen adora Pronus*; Silius Italicus 17.184: *effundunt lacrimas dextramaue ut numen adorant*; Pliny, N. H. 11.250; and other passages.

oscula, and obviously describes the future emperor throwing kisses with his hand in abject adulation of the rabble.

Thus both the Greeks and the Romans used for the obeisance before rulers words which were also used for the worship of the gods. The verb προσκυνεῖν was very probably chosen by the Greeks for two reasons: (1) the attitude of the Persians before their rulers reminded them of their own gesture when adoring Ge with prostration and a kiss; (2) since they thought that the Persians considered their kings gods, the Greeks and Romans attributed to the ceremony of the obeisance a similar spirit of reverence. Προσκυνεῖν therefore expressed both the gesture and the feeling which they attributed to their Oriental neighbors. *Adorare* was used regularly for the same ceremony at a rather late period. It is possible that this happened only after the verb had, perhaps through mistaken etymology, come to be associated with a hand-kissing gesture. It then seemed to be the exact translation of the Greek verb προσκυνεῖν.

RELATIVE AND DEMONSTRATIVE PRONOUNS IN OLD ENGLISH

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The relative pronouns in Old English are *þe* and *se, þe* being used when the clause has the function of a limiting or distinguishing adjective and *se* when it has the function of a descriptive adjective, e.g.:

Se casere þe wæs Claudius haten 'the Emperor called Claudius'.

Se casere, se wæs Claudius haten 'the Emperor, who was called Claudius'.

In the first sentence the relative clause distinguishes one particular emperor from others, in the second it adds a descriptive detail to a noun already sufficiently defined. Both kinds of clause may qualify the same noun, e.g.: Hom. I. 100.4 Se eahtateoða dæg þæs monðes þe we hataþ Martius þone ge hataþ Hlyda 'the 18th day of the month that we call March, which you call Hlyda'. The analogy of other subordinate clauses leads us to expect that conjunctive order[1] will be the rule in the relative clause, and this we find to be the case; but common order is a frequent exception especially when the verb is unstressed, e.g., 'sum mæsse-preost on þam lande þe is gehaten Hispania se wæs ðearle geswenct'. Occasionally the order of subject and verb is inverted as in 'Abel þone ofsloh Cain his broðor'.

In all the examples so far the antecedent has been a noun, and the order of words in the principal sentence presents no difficulty. The antecedent may, however, be the demonstrative pronoun, e.g., se biþ cyning þe rixaþ 'he that rules is King'; when this is the case, can any rule be laid down for the order of words in the principal sentence? The Homilies of Ælfric, which are by common consent the best example of mature OE prose, enable us to answer this question with complete certainty:

(1) If the pronoun-antecedent is the subject, it is followed immediately by the verb, e.g., II. 410 ft. Se ðe wyrcð mines Fæder willan se færð into heofonan rice 'He that does my Father's will goes into the

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[1] See my article in Medium Ævum 3. 3.

Kingdom of Heaven'.   II. 374.1 Se fandað þissera fif andgita seðe þurh fyrwitnysse hi aspent on unnyt 'He tries the five senses who through curiosity wastes them uselessly'.   II.48.7 þa ðe Johannes fullode þa wæron eft gefullode 'Those that John had baptised were again baptised'.

(2) If the pronoun-antecedent is in an oblique case, then (a) when the subject is a noun, we have demonstrative order, the verb standing next after the pronoun, e.g., I. 138.2 þone bær se ealde Symeon þe ealle ðing gewylt 'Simeon bore in his arms Him who rules all things'.   I.184.8 Ealle þa ðe him to cumað þa gesihð se Hælend 'All those that come to him the Saviour sees'.   I.190–22 þæt ðe on him heora eagan gedydon þæt deð ure geleafa on us 'What their eyes did for them, that faith does for us':

(b) when the subject is a pronoun we have common order, e.g., I.282.4 þæt þæt þu wylt þæt þu lufast 'that that thou willest thou lovest'. I.6.5 þa ðe his leasungum gelyfað þam he arað 'those who believe in his lies he honours'.   II.328.22 þone ðe Drihten lufað þone he ðreað 'whom the Lord loveth he chasteneth'.   These rules are supported by every other Old English text: exceptions like Oros. 56.24 Gecwædon þæt þa ðe ær æt þæm aþum næren þæt þa ham gelendon 'that those not at the oath-taking should return home' are only apparent, since the conjunctive order of the last clause is determined by the conjunction þæt.

We have now to ask ourselves whether the rules apply when the demonstrative pronoun is not the antecedent of a relative; in other words, can we detach the principal sentences in the passages cited and say, e.g., þa wæron eft gefullode 'they were again baptised', þone bær se ealda Symeon 'him old Simeon bore', þæt deð ure geleafa on us 'our faith does that for us'?   Such principal sentences abound in all texts as printed, and it will be necessary to examine the evidence for them most carefully.   Let us look at the supposed instances in texts which are translations from Latin and compare them with the Latin original:

(a) Principal sentences in which se (seo, þa) is the subject.   There are in Orosius (EETS Edn.) 19 instances; in 8 of these the Latin fails us, in the other 11 the Latin has either a relative clause or an adjectival phrase, e.g., apposition or participle, for which a relative clause is the natural equivalent in Old English (the antecedent is given in brackets):

40.17 (Se yldra wæs Danaus.)   Se wearð of his rice
           adræfed and on Arge fleonde becom
           Ipse Danaus, regno pulsus, Argos concessit.
78.26 (Htesseus) Se wæs mid his dædum snelra þonne
           he mægenes hæfde
           qui celeritate magis quam virtute fretus ...

82.18 (Marðonius) Se hiene wæs georne lærende
       Regem M. adgreditur, suadens ...
108.16 (Minutia) Seo hæfde gehaten þæt heo wolde
       hire lif on fæmnhade alibban
       (The rel. clause is a paraphrase of Virgo vestalis).
164.9 (Cartaina) Seo wæs getimbred fram Elisam
       quae ab Elisa condita invenitur
172.6 oþer consul wæs haten Cornelius: se gefor on Liparis
       C., consul alter, Liparam petiit
296.15 (Maximianus) Se wæs wierðe Romana onwaldes.
       Augusto dignus
 22.19 (Alpes) Ða onginnað fram þæm Wendelsæ
       quae Gallico mari exsurgentes ...
114.15 (On Tracia wæron twegen cyningas)   Ða wæron
       gebroþor
       Cum eum fratres duo, Thraciae reges, ...
278.15 (burga twa) Ða wæron on Tigris staðe
       duas Parthorum urbes cepit super Tigridem
288.22 (þa Seaxan)   Ða wæron eardfæste neh þam garsecge
       Saxones, gentem in Oceani litoribus sitam

Even where the Latin fails, a relative clause is always the construction
which naturally suggests itself, e.g.

 19.19 (micel sæ): seo is bradre þonne man oferseon mæge
192.20 (Hasterbale) Se wæs eac Pena oþer cyning
 72.1 (hund monna): þa wæron simbel binnan R. wuniende

What is remarkable in all these instances is that the OE *se* never renders
a Latin demonstrative pronoun: the reader must take it on trust that
the same is true of the very numerous instances in Bede.   There are,
however, in Bede rather frequent instances of a context (*ond se*) which
might seem to certify the demonstrative sense of the pronoun because
mere repunctuation is not enough to give the clause which it introduces
an alternative meaning, e.g., 376.31 wæs mid adle gestanden and seo to
þon swiðe weox ...   We have almost the same form of sentence in 382.4,
392.4, 462.3; in all but the last *seo* renders a Latin relative, and in two
of them, the first and third, one MS omits the *ond* or reads *ond heo*.   We
have also 50.3 and se mycele menigo heora fornom, and 148.9 and se
unrihtlice ofslegen wæs; for the first the Latin has quae tantam multi-
tudinem stravit, and in the second B omits the *se*.   In ASC we have:
716 A Her Osred wearð ofslegen and se hæfde VII winter rice (where E
omits the *ond*), and 755 butan anum gisle and se swiþe gewundad wæs
in all MSS, though just above (and se Cyneheard) E omits the con--

junction. Again, in Ælfric's Pentateuch, we have Gen. xiv.13 Ða
ætbærst sum man and se hit sæde Abrame   'Et ecce unus qui evaserat
nuntiavit Abram', and Gen. xli.34 and se sette gerefan 'qui constituat
praepositos'.

There is thus hardly a single instance of *ond se* which is not suspect, and
when we consider how easily the *ond*-sigil was inserted or omitted in
OE texts, suspicion is increased.   There are in Bede some half-dozen
instances of another strange use of *se*, in which the pronoun stands not
at the head of the sentence but within it, e.g., 100.17 Ða se þa geseted
wæs (B. þa se seonoð geset wæs) 'quod cum esset statutum', 258.29 wæs
se asponen from Cent (B. se wæs, O. Ca. wæs he) 'invitatus de Cantia',
326.17 þa frægn hine se 'a quo interrogatus', 370.3 wæs se in ealonde þæs
meres (B. Ca. se in ealonde wæs) 'qui in insula stagni' ... , 448.17 þa
se forðferde 'quo defuncto'.

Some of these look like an attempt to render the Latin idiom by which
the relative pronoun can be the subject of an adverbial clause, but
neither the Latin original nor the variant readings lend any support to
the view that *se* can be a demonstrative pronoun in a context where it is
not the antecedent of a relative.   Those who hold this view have, more-
over, a difficult semantic problem to solve, viz. what is the difference in
meaning between *se* and *he*?   It is a pertinent question, for there are
many passages like the following:

> Hom. II.96.19 He cuðe sumne man on Romebyrig. Se læg bedryda
> fram cildhade ... He læg singallice and næfre sittan ne mihte.
> L.S. 7.10 ... wæs sum æðelboren mæden. Seo wan ðurh geleafan
> wið ða feondlican ealdras. Heo wæs wlitig on ansyne ...

What is the distinction between *se* and *he* in the first passage, and be-
tween *seo* and *heo* in the second?   If our reasoning has been correct, there
is a syntactical difference, *se* and *seo* introducing a subordinate relative
clause, and *he* and *heo* a principal sentence, and the punctuation should
show this.

(b) We go on to consider supposed principal sentences introduced by
oblique cases of the demonstrative pronoun.   There are about 100
instances of these in Bede; for reasons of space I restrict myself to
twenty of them which illustrate the pronoun in all cases and genders,
assuring the reader that the remaining instances would only add to the
evidence of those cited.   They include examples both of demonstrative
and of common order (see 2a and b above) and I cite in each case the
opening words of the corresponding Latin sentence: 118.25 þæm æfter-
fyligde Justus in biscophade 'cui statim successit J.', 220.13 þane cwom

þider to halgianne Itthamar 'quem ordinaturus venit ... ', 394.16 þæs
wif wæs mid adle gestanden 'cuius conjux morbo ... ', 458.5 mid þone
wæron cumende oþre bisceopas 'cum quo et alii episcopi ... ', 476.3
æfter þam feng Aldwulf to þære ðenunge 'post quem A. suscepit episco-
patum', 478.11 For þæm wæs ercebiscop geworden Tatwine 'pro quo
factus est T. episcopus'.

The following all have common order: 154.6 (Ða com Eanfrid): þone he
gelice hlete geniðrade 'E. venientem simili sorte damnavit', 378.2 Ðæs
lichoman þa broðor ... 'cujus corpus fratres ... ' 332.17 þære bysene
heo wæs inhyrgende 'cujus aemulata exemplum ... ', 210.15 In þære
(gesyhðe) he wæs monad 'in qua admonitus est', 226.26 In þæm he
gesomnade micel weorod 'in quibus collecto exercitu ... ', 298.24 For
þæm Theodor Q. gehalgode 'pro quo consecravit ... ', 422.8 Bi ðære se
arwurða biscop cirican getimbrode 'in quo aedificata ecclesia ... '.

In the following we have plurals with common or demonstrative order:
184.10 þara wæs oðer biscop in Lindesse 'quorum prior in Lindissi
provincia ... ', 306.10 þara se ærra wæs ofslegen 'quorum prior occisus
est', 378.7 þa sume we geara for gemynde awriton 'e quibus aliqua
memoriae mandavimus', 88.31 In þæm wæron þa ærestan M. ond J.
'in quibus primi M. et J. ... ', 346.15 In eallum þæm he geornlice
gemde 'in quibus cunctis ... ', 214.22 From þæm he manig þing gehyrde
'a quibus non pauca audiit', 300.9 Mid þæm wæs eac Eadhæd gehalgod
'cum quibus et E. ordinatur'.

It will be observed that in all but one of these passages the supposed
OE demonstrative pronoun renders a Latin relative, and that in the one
exception (154.6) *þone* is most naturally construed as a relative pronoun.

(c) We have reserved for separate consideration the neuter pronoun
þæt. It is used as a demonstrative in the nominative case in all the
senses of the NE 'that', e.g., Hom. I. 6.26 Ic wille offon æt þe his blod
þæt is his lyre 'I will require of thee his blood, that is his perdition',
Hom. I. 42.13 Ðæt is ece iif þæt hi þe oncnawon soðne God 'that is
eternal life, to acknowledge thee as true God'.

There appears to be no reason why it should not be the subject of a
stressed verb also; instances are rare, and it is remarkable that whenever
it can be tested in texts translated from Latin it is the relative pronoun.
In Bede 200.28 Ðæt gelomp in þa tid þætte Penda ... its obvious con-
struction is as a relative after 'oðer wundor' in the preceding sentence:
'another marvellous event which happened at this time, viz. that Penda
etc.' Let us now turn to the oblique cases. Of the accusative there are
in Bede and Orosius ten instances with the appropriate word-order, e.g.,

Or. 38.25 þæt deð God to tacne, but in all but three the Latin fails.
Two of these three have what looks like demonstrative order: Or. 172.2
þæt gefremede Duulius hiora consul, Bede 374.15 þæt wolde ða æteawan
seo godcunde arfæstnes.

In the first the Latin has 'quod D. consul implevit'; in the second, since
the words begin a new Chapter, *þæt* is meaningless either as demonstra-
tive or relative. The Latin has 'volens autem', and one MS reads 'þa
wolde þa', which is obviously correct: 'since the Divine providence
wished to show'. In the third instance we have the common order
appropriate to a pronoun-subject (2b supra), but the Latin disallows a
demonstrative: Oros. 60.8 þæt ic wille eac gescadwislecor gesecgean
'quod utrum ita sit apertissime expediam'.

We have an apparent instance of the same order in Ælfric's OT: Gen.
xxxvii.22 þæt he sæde forðam ðe he wolde etc., where the Vulgate has
Hoc dicebat. Here *þæt* may be a misreading of *þes*, since 'þes he sæde' is
the regular phrase in Ælfric. Of the genitive *þæs* instances are very
rare except in the phrase 'þæs is to tacne' (Bede 264.11); by contrast
with 'þæs to tacne is' (116.16), this looks like a principal sentence 'of this
we have a proof', but the Latin for both forms alike is invariably
denique and the relative pronoun always gives the more appropriate
sense in both.

We can now summarize our conclusions as follows: No part of the
demonstrative pronoun *se seo þæt* other than the neuter nominative can
stand at the head of a sentence except as the antecedent, and any sup-
posed instance to the contrary is really a relative pronoun.

A simple repunctuation will usually make faulty sentences regular and
it will be appropriate, since we have used translated texts to prove our
rule, to illustrate from other texts the kind of remedy just mentioned:
Hom. II. 66.22 Babilonia is gereht gescyndnys. Seo getacnað helle
'B. is interpreted 'confusion', [which] betokens hell'. II. 296 ft. Se
apostel hæfde twa dohtra. Ða þurhwunodon on mægðhade 'The apostle
had two daughters, [who] continued in virginity'. II. 516.5 . . . preostas;
ða he wolde sibbian ær his forðsiðe. 'priests, [whom] he wished to re-
concile before his death'. I.532 ft . . . sum broðor. Ðam filigde sum
flæslic broðor 'a monk, [who] was followed by a certain carnal brother'.
It will not be amiss to classify the contexts in which this kind of mis-
punctuation is most frequent:

(i) The double relative clause. When the *se*-clause comes first, e.g.,
Is þæt þæt mynster, þæt is nu micel of medmiclum gefremed, þe gewune-
lice is Mingeo nemned 'this is the abbey, now greatly enlarged, that is
usually called Mingeo', the syntax as a rule is unmistakable; it is when

it comes second that mispunctuation occurs, e.g., Hom. I. 258.10 he geceas ða twelf leorning-cnihtas þe we apostolas hatað. Ða wæron mid him æfre siððan 'He chose the twelve that we call apostles, [who] were ever afterwards with him.' Oros. 19.7 ... to þæm porte þe mon hæt æt Hæþum: se stent betweoh Winedum ond Seaxum 'to the port called Heathby, [which] stands between the Wends and the Saxons'. Both relative clauses may be descriptive if sense requires, e.g., ASC 890 Goðrum þæs fulluht-nama wæs Æþelstan. Se wæs Ælfredes godsunu 'Guthrum, Alfred's godson, who had been baptised A.' Hom. II. 18.26 ðry cnihtas: þa wæron gehatene Annanias Azarias Misahel. Ða gecwædon ... 'Three young men [called] Ananias, Azarias and Misael, [who] said ... '

(ii) We have a special case of this after an interrogative sentence, e.g., Hom. I. 264.11 Hwæt is þæt rice buton ða halgan menn þe he alysde fram hellewite? Ða he betæcð his agenum Fæder 'What is the Kingdom but the holy men that he redeemed from hell-torment, [whom] he will deliver to his Father?' Hom. II. 280.15 Hwæt is þæs lambes innewerde buton Cristes bebodu? ða we etað þonne we lifes word underfoð. 'What is the lamb's inward part but Christ's commands, [which] we eat when we receive the word of life?'

(iii) Sentences introduced by the indefinite neuter þæt. If the sentence comes first it is, as a rule, unmistakably a relative clause, e.g., Hom. I. 318.26 ond eac, þæt wundorlicor wæs, ælcum wæs geþuht swilce he spræce mid his gereorde 'and further, what was more wonderful, he seemed to everyone to be speaking in his own language'. If it comes last, it is usually punctuated as a principal sentence, e.g., Oros. 172.2 Ðæt Duulius heora consul gefremede 'which [i.e. the building of a fleet] the consul D. accomplished'. Hom. I. 496 ft. He mæg ða synfullan sawle geliffæstan. Ðæt geswutelode se Hælend þaþa he þæt mæden arærde 'He can quicken the sinful soul, [as] the Saviour made clear when he raised the maiden.' Hom. I. 548.23 Sittende he tæhte: þæt belimpð to wurðscipe lareowdomes 'sitting he taught, [an attitude which] befits the magisterial dignity'. Blick. 123 mid. Ðæs we sceolon Drihtne þanc secgan 'for all [which] we ought to thank the Lord'.

(iv) Sentences introduced by a phrase of which the original pronominal meaning has been lost, e.g., þæs, forðam. In the Charters þæt also is used as an adverb, e.g., Ðæt west andlang burnan 'and so west along the stream'. Ðæs may be either adverb or conjunction, e.g., ASC. 871 Ðæs ymb iv niht 'four days after', 894 ymb twelf monað þæs hie comon 'twelve months after they had come';

and so forðam, which may mean either 'therefore' or 'because'. Bi ðam,

on the other hand, is only used as a conjunction ('as'), e.g.: Hom. I. 322.30 todælende æghwilcum be ðam ðe him gewyrð 'assigning to every man as seems to him good'

It is common in the Homilies and Cura Pastoralis in sentences like Bi ðam Johel se witga cwæð 'as Joel the prophet spake' introducing a quotation from Scripture.　Here it is usually rendered 'concerning this (or these)' but that is to make *ðam* a demonstrative pronoun, which is disallowed by our rule; there is of course no objection to its retaining its full force as a relative pronoun in a proper context, e.g. þa fule leahtras be ðam cwæð se apostel 'the foul sins of which the Apostle spake'.

We have reached the conclusions that the demonstrative *se* is never used except as antecedent, that the nominative *þæt* is freely used even when it is not the antecedent, and that any other part of the demonstrative pronoun cannot stand first in a sentence unless it is the antecedent. Internally, however, the oblique cases of the demonstrative pronoun in all genders are used freely, e.g., Hom. II. 18.15 An ðæra wæs Sibylla 'One of these (heathen witnesses) was the Sibyl'.　II. 480.26 Sy þæs Gode lof 'Praise be to God for that'.　II. 294 ft.　Aræraþ Cristes rode and gebiddað eow to ðære 'Raise up Christ's rood and pray to that.' II. 210.21 Hi offrodon lamb ond he wæs eac on ðam getacnod 'They offered a lamb and he was also symbolized by that'.

The doxologies in the Homilies show clearly the distinction between front position and internal position, e.g. demonstratives, II.528 us getiðað þæs, II.472 sy him þæs wuldor; relatives, II.332 þæs us getiðige, I.76 þæs him sy wuldor (which latter Thorpe also translates as demonstratives). Since *heora* precedes the word on which it depends, e.g., heora an, heora oðer (Oros. *passim*), we have the interesting series:

Oros. 88.19 Heora an wæs Claudius haten (personal pronoun)

Hom. II. 482.13 An þara hatte Simon (dem. pronoun)

Bede 184.10 Ðara wæs oðer biscop (rel. pronoun)

i.e. 'one of them', 'one of these', 'one of whom'.　The translators give 'one of whom', 'of these one', for the last two.

Apparent exceptions to our rule abound in verse: from considerations of space we confine ourselves to Beowulf and to those instances in the poem in the punctuation of which all the editors are agreed:

(i)　*Se* is made the demonstrative where it is not the antecedent:

194 [Higelaces þegn] Se wæs moncynnes mægenes strengest

898 [Wælses eafera] Se wæs wreccena wide mærost

1296 [anne] Se wæs Hroðgare hæleþa leofost

2024 [dohtor Hroðgares] Seo gehaten is | suna Frodan

2087 [glof] Sio wæs orðoncum eall gegyrwed

2391 [god cyning] Se ðæs leodhryres lean gemunde

3042 [legdraca] Se wæs fiftiges fotgemearces.

These are all relative clauses, the antecedents being given in brackets;
two of them (2024, 2391) have conjunctive order and must in any con-
text be relative clauses. Immediately after 898 we have the sentence
'he þæs aron ðah'; if *se* is a demonstrative pronoun, why the change to
he?

(ii) An oblique case is made a demonstrative, e.g.:

12 [god cyning] Ðæm eafera wæs æfter cenned

59 [Healfdene] Ðæm feower bearn ... in woruld wocun

1037 [mearas] Ðara anum stod | sadol searwum fah

1145 [hildeleoman] þæs wæron mid Eotenum ecge cuðe

1349 [ellorgæstas] ðæra oðer wæs | idese onlic

2194 [sincmaððum] Ðæt [?þe] he on Beowulfes bearm alegde

2769 [segn eallgylden] of ðam leoma stod

3014 [beagas] þa sceal brond fretan.

These again are relative clauses: all except two have conjunctive order.

(iii) *þæt* (nominative) is made a demonstrative when not followed
immediately by the verb:

1255   Ðæt gesyne wearð

2327   Ðæt ðam godan wæs
        hreow on hreðre

2709   Ðæt ðam þeodne wæs
        siðast sigehwil

The conjunctive order stamps each of these as some kind of subordinate
clause 'so that it was seen' etc. In others the presence of an obvious
antecedent points to the pronoun being the relative even when the
order allows the demonstrative:

309 [sæl timbred;] þæt wæs foremærost | receda

454 [hrægla selest] Ðæt is Hrædlan laf

1559 [eald sweord.]   þæt wæs wæpna cyst

1548 [breostnet broden;] þæt gebearh feore

The last is the only instance in the poem of a stressed verb following
þæt.

(iv) *þæt* (accusative) is taken as the demonstrative, e.g.:

194 Ðæt fram ham gefrægn Higelaces þegn

588   þæs þu in helle scealt
        werhðo dreogan

942   Hwæt! þæt secgan mæg
        efne swa hwylc mægþa swa þone magan cende

2479 Ðæt mægwine mine gewræcan

2629   ne his mæges laf
        gewac æt wige; þæt se wyrm onfand.

Only in 942 is *þæt* a demonstrative; here, however, *hwæt* is the headword
and it is followed, as always in the poem, by conjunctive order.   In 194
sense disallows even a relative, since the line begins a new paragraph;
þæt is a scribal error, as elsewhere, for *þa*, which gives excellent sense:
'when in his home afar he heard of Grendel's deeds, he bade prepare a
goodly ship'.   In the other passages *þæt* is the usual indefinite neuter
relative 'for which (i.e. the slaying of thy brother) thou must suffer
damnation (588)', 'which (i.e. their plotting of forays) my kinsmen
avenged (2479)', 'which (i.e. that the sword did not fail him) the dragon
discovered (2629)'.   In the last passage 'þa' is the MS reading, and 'swa'
would perhaps be a better emendation.

A word must be said about the exclamatory sentence-form Ðæt wæs
god cyning 'that was a good King!'   There is nothing like it in OE
prose, and even in poetry it is difficult to believe in such a neuter
demonstrative except with a defining clause.   Now in 11 and 2390 it is
followed by the lines cited above, Ðæm eafera wæs æfter cenned, and
Se ðæs leodhryres lean gemunde, the conjunctive order of which marks
them as subordinate.   The sense therefore is 'A good king was he, to
whom in after days a son was born, sent by God to solace his people'
and 'A good king was he, who in after days remembered to avenge his
lord's downfall'.   In each case the relative clause defines the demon-
strative pronoun.   In other contexts, e.g.:

1075   hie on gebyrd hruron
        gare wunde; þæt wæs geomuru ides.

1812   nales wordum log
        meces ecge.   Ðæt wæs modig secg.

the sentences are more naturally attached to the preceding one as con-
secutive clauses: 'fated they fell by the spear, so that the lady was sor-
row-stricken', 'he belittled not the blade, and a proud man was Unferth'.

The usual position in poetry of the neuter demonstrative in any
oblique case is after a pronoun-subject or, if the subject is not a pronoun,
after a governed pronoun, e.g.:

    632 Ic þæt hogode þa ic on holm gestah
        'This was my purpose when I put out to sea'
    798 Hie þæt ne wiston
      7   he þæs frofre gebad
    1774 Me þæs on eþle edwenden cwom
        'All that was changed for me'

The pronoun was no doubt proclitic to the stressed demonstrative as in 'he þa', 'he þær' etc., already discussed.[1] The reader will observe that the order Ðæt ic hogode (632) or Ðæt hie ne wiston (798) would be equally regular metrically, and the fact that it never occurs is a further argument that it was not good usage.   The only context in which *þæt* (acc.) is the headword is when it is followed by an unstressed verb, e.g.:

1700 Ðæt la mæg secgan se þe soð ond riht
     fremeð on folce

2864 Ðæt la mæg secgan se wyle soð specan

We have here the demonstrative order described in 2.a. at the beginning of this article: it may be due to the *la*, since interjections always influence word-order.

We conclude that, except for the sentence-forms mentioned in the preceding paragraph, the rules governing the syntax of the demonstrative pronoun when not the antecedent are the same in poetry as in prose.

I have not thought it worth while in this article to discuss the problem of the demonstrative pronoun, when not the antecedent, followed by conjunctive order, since that order is never found after the demonstrative as antecedent.   Instances, however, are numerous in our texts as printed, e.g.: Bede 40.14 Be þam þonne cuð is₁ 46.5 Ðone man nu todæg sceawian mæg, 50.14 Ðæt cuð is, 92.4 Se Bretta þeode fornom. Comparison with the Latin shows that every such sentence is the subordinate clause which the word-order leads us to expect, and that the supposed demonstrative is, therefore, as elsewhere, a relative pronoun.

MISCELLANEA

Manual J. Andrade poses (LANGUAGE 12.1–14) Some questions of fact and policy concerning phonemes. Those questions are entirely concerned with a monograph of mine upon the subject, and center on two main points: the accuracy of my remarks on the determination of phonemes by acoustic analysis, and certain metaphysical considerations.

On the first of these points, Andrade's reading of my monograph 17–25 has led him to an interpretation which is very far indeed from any communicative intent of mine. Although Andrade is the only reader, so far as I know, to reach this interpretation, it is possible that other readers, less well informed on acoustics than Andrade, may, as he fears, be misled. Lest the vigor of Andrade's protests create the impression of a controversy in terms of personal opinions and personal competence, I attempt explicitly to remove any basis for error in this regard.

I was concerned with a consideration of the criterion for combining various objectively different sounds into the same phoneme-classification. We all know that the stops of *tin*, *city*, *sit* are very different sounds; but we all believe that they somehow belong to the same phoneme. For reasons indicated 19 ff., I concluded that Bloomfield proposed a criterion for grouping together these different sounds: namely, that, though different, these sounds possessed in common some 'minimum same of vocal feature', some 'phoneme-feature in the sound-waves'. But it is a fact that acoustic analysis reveals nothing which all these [t]s have and which no [p] or [d] ever has[1]. Hence the grouping together of these [t]s into a single phoneme-classification cannot be based upon a 'phoneme-feature in the sound-waves'. Yet these [t]s, in the practice of every writer on phonemics, are considered as one phoneme. It follows, then, that phoneme-grouping is not a product of acoustic analysis; that various sounds may belong to the same phoneme without any demonstrable common 'phoneme-feature in the sound-

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[1] Andrade makes no attempt to deny this fact.

waves'. Hence, whatever acoustic similarity or constant may be found for SOME phonemes[2], here is an instance where none can be found. Whenever such similarities or constants may be found, they cannot be the basis on which phoneme-grouping is justified; for that basis is lacking for [t], which is none the less a phoneme. Therefore, such similarities or constants are not essential to phoneme-grouping; therefore, such similarities or constants in the sound-waves cannot be the 'phoneme-features'.

Andrade, approaching the subject with more interest in experimental phonetics than in phonemics, disregarded the fundamental distinction between phonemes and speech-sounds. Accordingly, in reading my conclusions, which were explicitly and exclusively concerned with the determination of characteristic 'phoneme-features', he interpreted those conclusions as a denial of the discovery of any similarity or constant for any SPEECH-SOUND. He writes: 'Twaddell seems to imply in some instances that all endeavors to analyze sound waves corresponding to speech sounds have been virtually futile (1)'. 'Twaddell claims that no constants have been found in successive oscillograms of the same sound (4)'. If that 'implication' seems, to any one else, to be implicit in my discussion of Bloomfield's 'phoneme-features', I explicitly disavow it here. If that 'claim' is made anywhere in my monograph, careful rereading has not revealed it to me; Andrade gives no reference. Since it is against that alleged 'claim' that Andrade chiefly protests, I can satisfy any legitimate demands of his by here stating that I should regard any such claim as contrary to the known facts, as inherently self-contradictory, and as wholly without bearing on the phoneme.

One major point of Andrade's criticism appears to call for comment. Because of the completely different purposes which he and I have in view, I fear that his remarks may give rise to an unjustified optimism, just as he feared the reverse from mine. He bases much of his argument on quotations and summaries from the descriptive work of Gemelli and Pastori[3]; and he gives the impression that these authors have found a 'phoneme-feature' for each Italian phoneme. This is, as yet, hardly the case.

In the first place, Gemelli and Pastori explicitly use the term *fonema* 'inteso in senso lato'; the definition in their text (79) leaves out of account the differential function and aggregational character of the phoneme. In effect, they regard 'fonema' as equivalent to 'single speech-sound':

[2] As in some of the cases mentioned by Andrade.
[3] L'analisi elettroacustica del linguaggio, Milan 1934.

'ogni suono o rumore del linguaggio, che non sia più scomponibile in altri elementi del linguaggio, ma solo in fenomeni oscillatori semplici'— something very different from the Bloomfield phoneme under discussion.

Andrade writes (4) at some length on the discovery of constants for Italian *a*; that these constants are none too definite is shown by the statement (104): 'I sopratoni di ampiezza massima possono essere due o tre; la loro frequenza può corrispondere a tre, quattro, cinque, sei, sette, e persino otto volte la frequenza del ciclo; di fronte a questa variazione del numero e della frequenza di tali sopratoni, *il solo fenomeno constante è lo smorzarsi periodico della loro somma.*' (Italics by G. and P.)

For the consonants, especially the voiceless stops, even less than this modest 'phoneme-feature' appears to be demonstrable. All the oscillograms here appear to be restricted to the medial occurrence of the stops, thus rendering no assistance in the problem of phoneme-grouping which was the issue of my exposition. And even with this great restriction, the description (145) falls far short of offering any characteristic 'phoneme-feature'. Cf. also 157: 'Così pure la *"t"* della parola *"mito"* (Tav. XXXVII) produce spostamenti bruschi della curva, senza alcun carattere di uniformità nè di periodicità.'

No attempt is made to discover oscillographic evidence for the fact that [n] and [ŋ] constitute a single phoneme in Italian.

The preceding discussion, I hope, will not be taken as a reflection on the value of the work of Gemelli and Pastori. They were concerned with the description of certain typical Italian sounds, not with the determination of phonemes[4].

With the metaphysics of the phoneme, Andrade's second major criticism, I am little concerned. My objections to certain phoneme-definitions are not based on any philosophical subtleties, but on the

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[4] Concerning the usefulness of the work for the determination of phonemes, consider the following extracts from a review by Treviño, American Speech 10.220 f.: 'Vowels have the greatest number of distinguishers, voiced consonants have fewer, while some voiceless consonants have none. . . . [a] is the most complex, while [i] and [u] are less so. . . . The vowels [e] and [o] are considered combinations of the three fundamental vowels because of the variability observed in them. . . . The technique employed apparently does not differentiate between open and close vowels. This does not substantiate the fine discrimination claimed for it. . . . The linguistic material consisted of isolated vowels, twenty-five words, and ten phrases. . . . In general, neither the linguistic material nor the type of subjects used seems to furnish a broad enough basis for the conclusions reached on the structure of speech.'

simple pragmatic consideration that the procedures seem to me not to work.   I used the term 'fiction', to which Andrade objects, to describe a relational, abstractional class, the sum of terms of differences.   If that use appears metaphysically unsound, I regret any difficulties it may have caused my readers.   The question of 'reality' vs. 'fiction' is, at all events, no invention of mine.   Gemelli and Pastori, for example, concede that it is a genuine question, though necessarily irrelevant for their work: 'la questione ... della reale esistenza del fonema ... è questione linguistica che esula dal campo delle nostre ricerche' (77).

I have above tried to answer the more important of Andrade's questions.   Many objections and replies to his less important points could be made if there were any profit or pleasure in picking flaws.   I content myself with this general observation: Following Gemelli and Pastori, and disregarding Bloomfield (who was under discussion), Andrade fell into a serious initial error of interpretation by considering 'phoneme' as equivalent to 'speech-sound'.   In consequence, some of the quotations in his Questions are partial or distorted, and unintentionally misrepresent the source; his summaries unintentionally attribute to my monograph statements and inferences not to be found therein.

<div align="right">W. F. TWADDELL</div>

THE ETYMOLOGY OF OP *vardanam*

OP *vardanam*, Skt. *vṛjánam*, and Av. *vərəzə̄nəm* are usually grouped together with the common meaning of 'enclosure, fortified place, town', to the Indo-Iranian root **varj-*, IE **u̯erĝ-* 'surround, enclose, protect'.[1] This assumption of close relationship seems at first sight to be justified by the exact correspondence of OP -*d*-, Av. -*z*-, and Skt. -*j*-, all < IE **ĝ*. The traditional viewpoint has continued dominant despite the sharp controversy over these words in the 1880's and 1890's[2]; even in Bloomfield's interpretation of *vṛjánam* as 'sacrificial plot'[3] the notion of 'enclosed (sacrificial) area' is underlying.

Of the three words, *vardanam* is the clearest with regard to its meaning, since from the context in all its occurrences it must mean 'town' or 'settlement'; whereas the exact meaning of *vṛjánam* and *vərəzə̄nəm* is not clear in all cases.   The phonetic provenance of *vardanam*, however,

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[1] Bartholomae, AIrWb. 1425; Uhlenbeck, Etym. Wb. 293; Walde-P. 1.290.

[2] Bartholomae, BB 8.218, IF 19. Beiheft 223; Foy, KZ 34.241 ff., KZ 35.24; Geiger, Civ. of E. Iran 2.3 f.; Geldner, BB 25.523, Ved. Stud. 1.139-54, 2.18 f., ZDMG 52.733 f.; Meringer, IF 17.154; Oldenberg, GGA 1890 410 ff.

[3] JAOS 48.219 ff.

is decidedly less certain than that of the other two words, since its -*d*-
may be from IE *-*d*-, *-*dh*-, *-*ĝ*-, or *-*ĝh*-.   Because of this possibility,
Ascoli (KZ 16.120) and Schmidt (Idg. Voc. 2.296) proposed to separate
vardanam from its supposed cognates, connecting it instead with the
Indo-Iranian root *vardh- 'grow' and with Latin *urbs*; -*rb*- < *-*rdh*-, and
the meaning 'town' in both cases supposed to have developed from 'the
growing one, the flourishing one'.   This etymology has not found gen-
eral acceptance (cf. Meringer, IF 17.157, Walde-P. 1.275; *urbs* to
verbera, verbēna) and a similar semantic development is not found in
any of the other words for 'town' in the IE languages.[4]

Yet there is another reason for assuming that the -*d*- in OP *vardanam*
is from *-*dh*- rather than from *-*ĝ*- or -*ĝh*.   In the Kathāsaritsāgara
(19.17; 24.71) and elsewhere (cf. Böhtlingk und Roth, PW 5.887) we
find the town-name *Pauṇḍra-vardhanam*; -*vardhanam* occurs also in
Puṇḍra-vardhanam (PW 5.757) and *Puṇya-vardhanam*[5] (PW 5.761).
This -*vardhanam*, obviously meaning 'town (of the Puṇḍras, etc.)', is
on the face of it the exact equivalent of OP *vardanam*, and their rela-
tionship would seem a reasonable assumption.   The element *vardh*-
is also found in the town-name *Vardhamāna-puram*, KSS 39.3, 124.105
and elsewhere (see PW 6.792 f.)

But, as has been set forth previously, a derivation from the super-
ficially clear connection, the root *vardh*- 'grow', seems unlikely on
semantic grounds.   While conscious of the inadvisability in general of
relating otherwise unconnected words in widely separated languages,
we nevertheless venture, on phonetic and semantic grounds, to propose
a connection between the *vardh*- seen in OP *vardanam*, Skt. -*vardhanam*,
Vardhamāna-, and Gmc. *ward- in OE *wardōn*, OHG *wartēn* 'keep

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[4] Words for 'town' have arisen chiefly from such sources as 'fortress' (the group
Skt. *pŭr, pŭram, puris*, Lith. *pilìs*, Grk. *pólis*; Welsh *dinas* : Gallic -*dūnum*, OIr.
dūn; cf. also Hung. *város* : *vár* 'castle'), 'enclosed place' or 'that which encloses'
(the Slavic group *gordŭ, usually regarded as borrowed from Gmc. : Goth. *gards*
'house' < enclosed place' etc.; NE *town* : NHG *Zaun*, ON *tūn*, etc.), 'place'
(NHG *Stadt* : OHG *stat* etc.; Pol. *miasto* : ChSl. *mĕsto* etc.), 'country estate'
(Fr. *ville* < Lat. *villa*), or 'citizenship, citizenry' (OFr. *citĕ*, It. *cittá*, Sp. *ciudad* <
Lat. *civitātem*); but none from the idea of 'growing' or 'flourishing', which seems
altogether too sophisticated and poetical and also too much of an ex post facto
notion.   For the relation of ChSl. *mĕsto*, etc., to an IE *meit- 'fat, nourishment'
see Walde-P. 2.247.

I am indebted to Prof. C. D. Buck for criticism and for permission to use for the
above note his unpublished Dictionary of Selected IE Synonyms.

[5] Which may be an error for *Puṇḍra-vardhanam*. *Pauṇḍra-vi-vardhanam*
(PW 5.887) may also be an error for *Pauṇḍra-vardhanam*.

watch over, protect', NE *ward*, Goth. *daura-wards* 'gate-watcher', etc;
IE **u̯er-dh-*. Hence *vardanam, -vardhanam* with **-eno-* suffix, and
Vardhamāna- (pres. pass. pple.) would mean 'the protected, guarded
place > town'.

Therefore the traditional grouping should be split, and two groups
set up: Skt. *vr̥jánam* and Av. *vərəzānəm*, to the root **varj-*, IE **u̯er-ĝ-*;
and OP *vardanam*, Skt. *-vardhanam*, *Vardhamāna-*, to IE **u̯er-dh-*.
The IE roots may in their turn be simply different extensions of the
same root **u̯er-* 'cover, shut in, protect' (Walde-P. 1.280).

<div align="right">ROBERT A. HALL, JR.</div>

BOOK REVIEWS

The Evolution of Malayalam Morphology. Pp. xix + 155. By
L. V. Ramaswami Ayyar. Ernakulam: Cochin Government Press,
1936. (Rama Varma Research Studies 1. Published by Rama Varma
Research Institute, Trichur, Cochin State.)

Comparative Dravidian studies have long been handicapped by the
lack of many fundamental works on the separate Dravidian languages.
The non-literary languages have to a great extent not been accurately
described; some, including the languages spoken in such accessible
areas as the Nilgiris, have not been described at all. Grammars of the
languages which possess literatures are available, though these are
generally descriptions of the modern standard and the ancient literary
languages, and vernacular dialectal studies are still to be made. It is
now well known in the case of the Sanskritic literary languages of north
India that scarcely one of them represents a vernacular pure and simple;
there have been all sorts of cross influences between literary traditions,
the most influential being of course that of Sanskrit itself, and archaizing
tendencies within each tradition have been stronger in this area than
probably almost anywhere else in the world. That such tendencies
work as strongly in South India has perhaps not been so vividly realized
by the scholars, for the most part Indian, who have worked on the
Dravidian literary languages. Or, if it has been realized, it has not
resulted in the production of authoritative historical grammars of the
separate languages evaluating the various tendencies outlined above, a
most necessary preliminary to valid comparative work in this field.
Much spade work must be done first, and is being done by European-
trained scholars and their pupils and by others; mention may be made
especially of the works on the historical grammar of Kannaḍa issuing
from Mysore.

The work under review is a notable contribution of this kind. It
traces the morphology of the Malayalam language from its first emer-
gence in literature and inscriptions down to the present day. If a fault
is to be found, it is the somewhat unsystematic way in which modern
vernacular forms are introduced; this is pardonable considering the
meagre materials that are at hand. Prof. Ramaswami Ayyar has
already dealt with phonology to some extent in earlier papers, and he
promises a treatment of syntax later. A comprehensive treatment of

Tamil and Malayalam is a desideratum which we may hope that he will also supply.

The headings of the treatment in the body of the present work are: nominal inflexions, demonstratives, pronouns (i.e. 1st and 2nd personal and the 'reflexive' pronouns; the separation of demonstratives from these is of course justified descriptively for Dravidian speeches), verbs, miscellaneous Malayalam formations, the language of some Malayalam classics, affiliations. While the bulk of the work is taken up with a strict detailed and documented account of the changing use of forms, there is much interesting interpretation of the detailed phenomena. One may mention especially the discussion of the historical disappearance of personal endings in the verb and the refutation, thoroughly sound, of the theory that the state of things in modern Malayalam represents a primitive Dravidian feature. It is here especially that one can see Prof. Ramaswami Ayyar's careful evaluation of the influence exerted by a foreign idiom (viz. Tamil) upon Malayalam literature. Very suggestive too is his treatment of the aoristic tense with -$\bar{u}$ and its origin.

Of most general interest is the reasoned conclusion (chapter on affiliation) based by the author on this work and on his work on Tamil (only partly published; especially 'Morphology of the Old Tamil Verb' in Anthropos), that within the Dravidian languages we must set up Tamil-Malayalam as a separate sub-group. The speech of the Malabar coast during the Sangam period of Tamil literature (until about the 5th cent. A.D.) is not represented by documents. The next period of the linguistic development of Tamil is seen in the poems of the Aḷwars and the Śaivite saints, our author's Early Middle Tamil (about 5th to 10th centuries). The early documents of Malabar, in 'Early Old Malayalam', a stage lasting until the 12th or 13th century, show a language already somewhat differentiated from Tamil but obviously very closely related to Early Middle Tamil in most of its forms and showing only a few features unrepresented in Tamil, and even these more closely allied to Tamil forms than to those of any other Dravidian speech. From this period on Malayalam goes its own way and develops as a distinct language independent of Tamil. While this in general outline has been accepted by most scholars since Gundert, we now have the outline filled in with more exact details. It is to be hoped that Prof. Ramaswami Ayyar's conclusions will prove cogent in convincing of error those scholars to whom he refers as maintaining that 'Malayalam is as much independent of Tamil as Kannaḍa or Telugu.'

<div align="right">M. B. Emeneau</div>

The Syntax of the Genitive Case in Aristophanes. Pp. xvi + 235; bound in buckram. By JAMES WILSON POULTNEY. Baltimore: The Johns Hopkins Press, 1936.

The publication of this considerable volume, the author's doctoral dissertation, was aided by a contribution from the Rockefeller Foundation. In it Dr. Poultney has classified all the genitives in the complete plays and the fragments of Aristophanes, nearly 5000 in number. In a few instances, where there might be real differences of opinion, he has given a translation or explanation to justify his classification. He uses the Oxford text, citing variant readings only when they introduce genitives of a different category. But limitations of space have in general prevented other than absolutely necessary comments.

The examples are classified into twelve chapters: Possessive Genitive; Genitive of the Author; Subjective Genitive; Objective Genitive; Various Adnominal Genitives (appositional, definition, measure, origin, predicate gen. of description); Partitive Genitive; Genitive of Content and Genitive of Material; True Genitive with Verbs and Adjectives; Ablatival Genitive; Genitive Absolute; Genitive with Prepositions; Genitive with Compound Verbs. Appendixes list the examples of Attraction of the Relative into the Genitive Case, and The Articular Infinitive in the Genitive Case. The volume closes with Bibliography, Addenda et Corrigenda, and an elaborate Index of the passages listed.

On page 47 the term Appositional Genitive is used. I regret this, since the term 'apposition' properly denotes that which is placed alongside some other substantive, whose construction it shares. Consequently 'appositional genitive' is a contradiction of terms, which might well be replaced by the 'genitive of equivalence'. The term 'genitive of definition' is used on page 49, for a very slightly differentiated idea, such as that in 'the plain of Marathon'; I wonder if the separation into two categories is justified.

On page 85 Poultney discusses briefly the distinction between those genitives which represent the IE genitive and those which represent the IE ablative. It seems to me that the distinction cannot in all instances be made with certainty; notably the partitive genitive contains many features of the ablative, seen more easily when one compares the Latin usages, as in *militum pars*, with the genitive, but *unus de multis*, with a phrase of ablatival meaning. Must we not admit for Greek that the 'partitive genitive' is a conflux of the two usages?

The volume is a valuable handbook for the study of Aristophanes, and of Greek syntax in general; the author is to be congratulated.

ROLAND G. KENT

On the Designation-Problem of the So-called Tokharian Language.
By N. FUKUSHIMA. Pp. 7–72, reprinted from the Memorial Volume
dedicated to the late Professor Katsuji Fujioka, 1935.

Professor Fukushima discusses in detail the various names which have
been proposed for Tocharian, in its two dialects, recovered for linguistic
science from the sands of central Asia. From the fact that the A-dialect
was called *ārśi-kāntu* by its users, he declares himself in favor of calling
the two dialects Arshian A and Arshian B; and he interprets *ārśi* as an
assibilated form of (dial. A) *ārki*, (dial. B) *ărkwi* 'white', belonging to
the IE root *arĝ* 'shining, whitish', seen in many words for 'white' and
'silver'. This may then have been their designation for themselves as
distinct from the (yellow) Asiatics of the region: the Karasharian kings
(dial. A) have names usually ending in *-arjuna* ('white' in Skt.) and the
Kuchean royal family (dial. B) is called Po ('white' in Chinese) in the
Chinese historical documents.

A bibliography, with summary of contents, fills pages 45–72. A laid-
in leaflet gives Addenda and Corrigenda, and an Index of the Authors'
Names.

But is not the term Tocharian so firmly fixed in usage that there is no
profit, but only confusion, to be got from urging a change?

ROLAND G. KENT

Les Infinitifs Avestiques. Pp. 117. By E. BENVENISTE. Paris:
Adrien-Maisonneuve, 1935.

For a long time I have been of the opinion that the mass of hetero-
geneous forms listed as infinitives by Bartholomae in cols. 1989–93 of
his Altiranisches Wörterbuch could not really all be infinitives. This
attitude on my part is reflected in the dissertation of my student, Maria
Wilkins Smith, Studies in the Syntax of the Gathas of Zarathushtra
(Language Dissertation No. 4 of the Linguistic Society of America,
1929). It therefore gives me great satisfaction to find that such an
able scholar as Benveniste has investigated the subject thoroughly, and
brought order out of chaos.

For of the about 240 forms listed by Bartholomae, he finds, on careful
examination of the passages, that only 60 deserve classication as infini-
tives. The other 180 (genitives, ablatives, accusatives, instrumentals,
locatives, datives in form) are subjected to individual analysis in their
context; most of them turn out to be ordinary abstract nouns, often with
dependent genitives. Some are finite verb-forms, a few are participles,
a few remaining forms are in passages too corrupt for definite decision.
In the process, numerous textual improvements are suggested; two or

more homophonic roots listed by Bartholomae are reduced to unity; the alleged adj. *drauga-* (Yt. 19.33-4; page 51) is found to be a substantive; etc.

With page 61 Benveniste begins the examination of the authentic infinitives, first those with recognizable case-forms (61–71), then those ending in *-dyāi* (72–100). Of the former he admits one instrumental, *apayeitī* Yn. 32.11; four (dubious) locatives, *dārayō* Yn. 32.1, *savayō* Yn. 51.9, and two from later sections; and 33 datives, almost all from the Gāthās except the 5 in *-tōe* or *-tayaēča* (from *-tayai*). Thus, apart from the last small group, these forms are all old; and all but those datives formed directly on the root show under his examination clearly their older substantival use.

The 22 infinitives in *-dyāi* (from Aryan *-dhyāi*) are divided into 8 formed on the thematic tense-stem, and 14 on the non-thematic (*srūidyāi* and *sūidyāi* are omitted in the enumeration on page 74). These form no part of any identifiable noun-formation, and must be regarded as the oldest type of prim. Aryan infinitives. There are 35 such forms found in the Rigveda, all built on a thematic stem. The formation starts therefore in the non-thematic stems, from which a partial transfer to the thematic stems is seen in the Avesta, and a complete transfer in the Rigveda: a striking example of the value of Iranian testimony on origins. Such forms are moreover found to be medial or medio-passive in meaning (despite the inherent difficulty in identifying the voice-meaning of an infinitive). These forms, both in Avestan and in Vedic, have certain features in common: they never govern a dative (except RV 6.1.1, a late passage); they are not used in the predicate after a negative; they may be used as imperatives; they have in their uses many affinities with forms having personal endings.

The final chapter (101–12) compares with these forms the infinitives of Modern Iranian. Persian has only a form in *-tan* or *-dan*, proceeding from the *-tanai* seen in Old Pers. *-tanaiy*, which is entirely lacking in Avestan; certain other dialects have the derivative of *-tayai*, seen in the later Avestan; other modern forms are sporadic.

There are many other interesting and valuable points made in this volume. Here, as in other studies of recent date, we are led to see the inadequacy of Bartholomae's monumental Wörterbuch; may we hope that presently we shall have from Benveniste the volume which must some day supersede it?

ROLAND G. KENT

The Ab Urbe Condita Construction in Latin. Pp. 81. By OTTO WILLIAM HEICK. University of Nebraska Doctoral Dissertation. Privately published at Lincoln (Neb.), 1936.

The phrase *ab urbe condita* is conventionally taken as the naming-example of the type consisting of noun and participle in agreement, where in English we use an abstract noun (Heick uses 'participle' for 'gerund' on page 67). Examples are listed from the Latin authors of the early and classical periods, though the examples from Cicero are intentionally only a selection, and those from Livy have been omitted because of previous monographs by other scholars.

The author says (9): '... the participle ... has replaced the verbal noun ... and conveys the leading thought ... *ab urbe condita* = *ab urbis conditione*.' Is this historically true? Apart from the fact that the example is unfortunate, since *conditio* is a post-classical word, an examination of the passages cited from Plautus on page 13 indicates rather that the peculiarity is in the English idiom, not in the Latin; I indicate the logically subordinate function of the participle by placing its translation in parenthesis:

Amph. 1038 *quid med advocato opust?* 'why do you need me (as advocate)?'

Bac. 219 *quod des inventost opus,* 'you need some (found) money'.

Stich. 632 *nunc consilio capto opust,* 'now we need a plan (decided on).'

Others are similarly easy of interpretation. The idiom seems to me to be quite a normal way of speaking, when the noun expresses the main idea as here. The phrase Trin. 305 *ab ineunte aetate* is like *ab imo monte* 'from the bottom of the mountain', where the adjective indicates a portion only of that which it modifies: 'the early part of his life'.

In such instances, accordingly, the participle indicates a limitation upon the noun, as does any adjective. English happens to make the qualification more prominent in the expression which is used; but the emphasis on the verbal noun of action is seen in Poen. 1308 *quid tibi hanc digito tactiost?* In actual fact, *urbs condita* is only the substantival correspondent to the sentence *urbs condita est* (cf. *equus albus* and *equus albus est*); this may be used in any case, and when the sentence is used in the ablative (the 'ablative absolute') we have *urbe conditā*. There is the same relation in *urbs capienda, urbis capiendae, ad urbem capiendam.* Heick's view (65) that the construction developed out of the ablative absolute fails to convince me; a more important clue lies in the original fewness of verbal nouns of action (rather than abstracts, p. 69), and the general concrete character of Roman expression (mentioned

69).   For a fair treatment of the subject, it would be necessary to list the verbal nouns with the genitive (or other construction), found in the earlier writers; a comparison would be illuminating.

Incidentally, I fail to find among the citations Vergil, Aen. 1.27 *spretaeque iniuria formae* 'and the insult to her beauty (which was scorned'), an excellent example of my point: for *spretae* could be omitted without changing the sense.   I can see no real difference between this and *rapti Ganymedis honores* in the next line, in which, however, nobody could use an English gerund in the translation.

The listing of the examples of this idiom—for it did develop into an idiom in Latin, in spite of what I have said as to its origin—is a worthwhile service, and I wish that Heick might investigate further along the lines which I have indicated.

ROLAND G. KENT

Introduction to the Study of Modern Greek.   Pp. 86.   By NICHOLAS BACHTIN.   Cambridge: Deighton Bell and Co., 1935.

Not a beginner's book, but an induction into the proper attitude toward the language, before taking up its acquisition as a spoken medium.   The author believes profoundly that every teacher of or scholar in Ancient Greek should be well acquainted with the Modern Greek; that the two should not be kept apart in our learning.   With this view I am in complete agreement; only, it is sometimes difficult to see how the extra knowledge may be attained: time or facilities may be lacking.

It is perfectly true that many details of ancient Greek can be understood only by comparison with modern Greek, and vice versa; but I suspect that a knowledge of ancient Greek is not helped so much by a knowledge of the modern, as that of the modern by a knowledge of the ancient.   Historical sequence favors this attitude.

Now in this paper-covered volume Bachtin attempts to set forth the main lines of development of the Modern Greek from the Ancient, in sounds, forms, and vocabulary.   Here and there I must take exception to his statements, though he knows Modern Greek far better than I ever did.   The vowel η can hardly have become a long close vowel as early as he states (21).   The velar spirant represented by γ before back vowels is found in dialectal German, in Dutch, and probably elsewhere, among IE languages.   The pronunciation of θ as [t] after [s] is not mentioned (37).   The pronunciation of voiced stops after nasals is disregarded in a generalization (38), though often mentioned elsewhere.

The survival of a pitch elevation on the accented syllables of Modern Greek is misinterpreted (39); as in English, it exists, but is not functionally significant. There is in real colloquial Greek more loss of syllables than Bachtin indicates (39); cf. πᾶς for ὑπάγεις, παιδί for παιδίον, and the list in LANG. 7.187. He fails to mention the partial replacement of the genitive by ἀπὸ and the accusative (43). Semantics seem to be to him a mysterious field in which any 'accident' may happen (66–7); in reality, the possibilities in this field are reducible to formulas, as readily as the phonetic changes. But we cannot predict which of several possibilities will be realized. My own impression is that there are more borrowed words in Modern Greek than Bachtin indicates (67), and so there are in French (68), especially in recent years.

In his final chapter (77–86), Bachtin discusses the struggle between the Koine and the Katharevousa, espousing the cause of the Koine. But the real popular speech is a grade socially far below that which is regarded as the Koine; and its users would never demean themselves by talking it to a foreigner, while the foreigner will give offense if he uses the same vulgar speech which is employed by his interlocutors among themselves. Is not the solution a compromise?

ROLAND G. KENT

An Introduction to the Ibo Language. Pp. xiv + 215. By IDA C. WARD. Cambridge: W. Heffer and Sons Ltd., 1936.

Ibo is a district on the lower Niger immediately above the delta, and mainly on the eastern bank. The language of Ibo is widely used in the region, in several dialects. Miss Ward had the opportunity of recording it from several native speakers, both in Nigeria and in London. Her object in the present volume is not to supersede the larger grammars of R. F. G. Adams and of Spencer, but to make a careful study of the tones which are essential elements in the speech; thus *akwa* with two high tones means 'cry', with high + low 'cloth', with low + high 'egg', with two lows 'bridge'. These tones are moreover not invariable in the word, but are subject to certain shifts according to the use of the word in the phrase or the clause. Of these tones there are five levels, as well as a glide from higher to lower; they are indicated by horizontal or slant lines in parentheses after the words.

There is a kind of vowel harmony, in that a word of two or more syllables (with a few exceptions) is limited to vowels of the same set; but these are not defined as front and back sets. Instead each set has the alternate vowels, starting from close and working to the open

position, in the two (front and back) series. Set one has close *i* and *u*, open *e* and medium *o*; set two has close *e* and open *u*, normal *a* and open *o*.

There are abundant exercises for the practice of the tones, which is the main purpose of the volume. I miss however a careful analysis of the sounds such as is given by Sapir, LANG. 7.31–41, for Gweabo. For example, only a few of the consonants are explained in detail; those which are explained are interpreted in terms directed only at a speaker of English, as for example the writing *gh* for what I judge to be the voiced velar spirant, weakly sounded, as in dialectal German *sagen*—I cannot feel sure from the description. There is no mention of aspiration of stops, or lack of it; there is no statement whether the vowels in succession (which are very common) are separated by a glottal catch or the like. For a volume which is intended to develop the pronunciation this seems to me a fairly serious defect. But if one can acquire the sounds from another source, I should judge that the account of the tones is adequate and the drill by the exercises would be effective.

ROLAND G. KENT

Etymology of Early Legal Terminology. Reprint of The Institution of Property, Appendix I, 539–82. By C. REINOLD NOYES. New York: Longmans Green and Co., and London: Humphrey Milford, 1936.

The author attempts to establish the original meanings of certain legal terms, by combining the previously expressed etymologies with a certain amount of demonstrable recorded meaning and his own personal predilections. The words treated are: A, Terms for the House, or Village, Community (punctuation sic!) and for the House Master (*familia, vicus, domus, pater, tribus, curia, gens, civis, locus, heres*); B, The Etymology of "Manus-Mund" and of the "Manus"-System; C, *vindex*; D, Absolute and Dependent Property (*patrimonium*, [Germ.] *alodium, catalla, pecunia, peculium*); E, The Terms of Law (*ius, lex, antiquo, veto, res*, Fr. *chose*, Germ. *Sache*, Eng. *thing*); F, The Feudal Terms (*dominium, medianus, in capite*). Derivatives are included with each.

A reading of this interesting pamphlet makes the impression that Noyes is not at home in etymological procedure. For example (541), after a résumé of various theories expressed on *familia*, he says: 'The evidence is of the slenderest and the conclusions are very uncertain. But we may gather that the sense of the German *gewohnen—wohnen*

which pervades most of the suggested sources, appears the most credible core idea for the group—the idea of "settled".' He therefore suggests as original meaning of *familia* 'those who customarily dwelt together', or 'a settlement'. He rejects (544–7) the certain derivation of *tribus* as a compound of the numeral *tri-*, and wishes to associate it with Oscan **tríïbúm** (which means 'house'). Other similar remarks on the discussion of other words could be added; I regret this, because I believe in the collaboration of scholars in two fields. But the present treatise should have been revised by a competent linguistic historian before it was put into printed form.

ROLAND G. KENT

NOTES AND PERSONALIA

THE LINGUISTIC SOCIETY OF AMERICA will hold its Thirteenth Annual Meeting on December 28–30, at Chicago, jointly with the American Philological Association and the Archaeological Institute of America. Headquarters and sessions will be at the Hotel Morrison, Clark and Madison Sts. Members who have not received the First Circular of the meeting should write at once to the Secretary of the Society.

PROFESSOR J. WHATMOUGH and PROFESSOR W. F. TWADDELL, delegates of the LINGUISTIC SOCIETY OF AMERICA to the Fourth International Congress of Linguists, held at Copenhagen August 26 to September 1, 1936, present the following report:

Despite the troubled clouds that overhang Europe, the Congress attracted a large number of scholars, from nearly all European countries, and elsewhere. North America was represented by at least eleven persons, of whom five were members of the Linguistic Society: Bernard Bloch, John Kepke, Kemp Malone, and the Society's two delegates. The papers read by the Society's members were as follows: The Development of Indo-European Labiovelars, by J. Whatmough; A Phonological Analysis of Intervocalic Clusters in German, by W. F. Twaddell; Postvocalic r in New England Speech, a Study in American Dialect Geography, by Bernard Bloch. One session of Section III A (Historique: Histoire d'une seule langue) was presided over by Kemp Malone.

Not only was the Congress a model of organization: it aroused much interest and enthusiasm; it was a great opportunity for scholars to meet and become something more than mere names to one another; and it was held in a city of singular interest and delight. Those who were privileged to attend will have pleasant and enduring memories of an altogether successful gathering.

But the most important event of the Congress, we think, was the showing of Roentgen-ray motion pictures, with sound, made by Prof. Paul Menzerath and Prof. Janker of Bonn. These films evoked great interest; the extraordinary clarity with which they demonstrate the movements of the vocal organs during speech convinced us that the

L. S. A. will be lacking in its duty if it does not provide for the showing of the film at its next meeting.

Professor Twaddell adds the following note on a special assembly at which an International Association for Phonology (Phonemics) was organized: Professor J. Vendryes served as chairman and announced that competent legal opinion indicated the desirability of organizing separate national groups rather than an international society, for the advancement of phonological (phonematic) studies. Certain general regulations were adopted, in conformity with whose provisions the national groups are to be organized, with a minimum annual membership fee of ten French francs; the Association will thus exist only as a totality of the separate national groups, holding normally a joint general meeting along with the International Congress of Linguists. For the period 1936–9, the following Executive Committee was elected: J. Vendryes, president; N. Trubetzkoy and R. Jakobson, secretaries; V. Mathesius, treasurer; V. Bröndal, G. Devoto, J. van Ginneken, D. Jones, J. Kurylowicz, E. Sapir, A. Sommerfelt, L. Weisgerber. Among projects discussed, and referred to the Executive Committee for action, were a revised edition of Trubetzkoy's Anleitung zu phonologischen Beschreibungen, and the preparation of an Atlas of Phonological Systems, at least in part, of the languages of the world.

MARK HARVEY LIDDELL, Professor Emeritus of English at Purdue University, and a Signer of the Call which led to the Foundation of the Linguistic Society of America, died at Orleans, Mass., on July 28, aged seventy years.

He was born on April 1, 1866, at Clearfield, Penna. He received the degree of A.B. from Princeton University in 1887, and later studied at Oxford and at Berlin. His teaching career included positions at the University of Texas, the University of Louisville, Butler University, and Purdue University, to the last of which he went in 1913. He was Professor of English there from 1921 to 1932, when he retired in order to give his entire time to research in acoustics and phonetics.

He was an active member of the Linguistic Society during the first six years of its existence, but then withdrew from membership. His scholarly activities are manifest in his numerous published volumes: editions of selections from Chaucer and of certain plays of Shakespeare, and researches in the fields of prosody and phonetics.

DR. EDITH F. CLAFLIN was Instructor in Latin in the 1936 Summer Session of Columbia University, and during the present academic year is giving a course on Mediaeval Latin in the University Extension department of the same institution.

DR. EVA FIESEL, Research Assistant in Etruscan at Yale University, has gone to Bryn Mawr College as Visiting Professor of Linguistics.

DR. ALBRECHT GOETZE, for two years Visiting Professor of Assyriology at Yale University, has been appointed Laffan Professor of Assyriology in the same institution.

DR. E. ADELAIDE HAHN, Professor of Latin and Greek in Hunter College, has been appointed head of the Department of Classics in the same institution, succeeding DR. ERNST RIESS, who has become Professor Emeritus.

DR. FRITZ MEZGER has been promoted from the rank of Associate Professor to that of Professor of German, at Bryn Mawr College.

RUSSELL B. NYE has gone from Jordan College (Menominee, Michigan) to the University of Wisconsin, as an Assistant in English.

DR. HAROLD D. ROSE's monograph, A Semantic Analysis of Time with a Semantic Alphabet of the Commonest English Words, pp. 15, has been published by The Indiana University Press. A limited number of complimentary copies are available, and will be sent to those who write to the author (Bowling Green State University, Bowling Green, Ohio, U.S.A.).

DR. MARIA WILKINS SMITH, who prepared the INDEX TO LANGUAGE I–V and the INDEX TO LANGUAGE VI–X, has asked to be relieved of this duty, which has won her the gratitude of all users of LANGUAGE. In succession to her, DR. GEORGE SHERMAN LANE of the Catholic University of America has consented to prepare the INDEX TO LANGUAGE XI–XV.

THE FOLLOWING NEW MEMBERS were received into the LINGUISTIC SOCIETY subsequent to the last published list, and up to September 11, 1936:

Charles W. Bliss, Loomis School, Windsor, Conn.
Margaret M. Bryant, Ph.D., Asst. Prof. of English, Brooklyn College; 604 W. 112th St., New York City.

Rufus S. Day Jr., A.B., LL.B., Attorney-at-Law, 1503 Midland Blg., Cleveland, Ohio; *Indo-European.*

Arthur R. Dunlap, Ph.D., Instructor in English, Univ. of Delaware; 6 Purnell Hall, Newark, Del.; *Middle English dialects, Indian place-names in Delaware.*

Harry L. Levy, Ph.D., Instructor in Latin and Greek, Hunter College; 3140 Netherland Ave., New York City.

C. Arthur Lynch, Ph.D., Asst. Prof. of Greek and Latin, Brown Univ., Providence, R. I.

Clarence L. Meader, Ph.D., Prof. of General Linguistics, Univ. of Michigan; 2022 Angell Hall, Ann Arbor, Mich.; *biolinguistics, Russian.*

Alfons Nehring, Ph.D., Schellingstr. 5, Würzberg, Germany; *Indo-European antiquities* (for 1937).

Mrs. Manuel E. Pablo (Winifred O'Connor), A.M., Assoc. Prof. of English, Univ. of the Philippines; 2960 Herran, Manila, P. I.

Walter B. Pillsbury, Ph.D., Prof. of Psychology, Univ. of Michigan; Psychological Laboratory, Univ. of Michigan, Ann Arbor, Mich.; *psychology of language.*

H. Helen Shohara, Ph.D., Instructor in Phonetics, Univ. of Michigan; Dept. of Speech and General Linguistics, Univ. of Michigan, Ann Arbor, Mich.

Edna Rees Williams, Ph.D., Instructor in English Language and Literature, Smith College; Dickinson House, Northampton, Mass.